An Address
ON
THE NATURE, ORIGIN, AND MAINTENANCE OF LIFE.

DELIVERED AT THE MEETING OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, HELD AT DUNDEE, SEPTEMBER 4TH TO 9TH, 1912.

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[AFTER some introductory remarks Professor Schäfer proceeded:]

Everybody knows, or thinks he knows, what life is; at least, we are all acquainted with its ordinary, obvious manifestations. It would therefore seem that it should not be difficult to find an exact definition.

Definition.

The quest has nevertheless baffled the most acute thinkers. Herbert Spencer devoted two chapters of his *Principles of Biology* to the discussion of the attempts at definition which had up to that date been proposed, and himself suggested another. But at the end of it all he is constrained to admit that no expression had been found which would embrace all the known manifestations of animate, and at the same time exclude those of admittedly inanimate, objects.

The ordinary dictionary definition of life is "the state of living." Dastre, following Claude Bernard, defines it as "the sum total of the phenomena common to all living beings."[1] Both of these definitions are, however, of the same character as Sydney Smith's definition of an archdeacon as "a person who performs archidiaconal functions." I am not myself proposing to take up your time by attempting to grapple with a task which has proved too great for the intellectual giants of philosophy, and I have the less disposition to do so because recent advances in knowledge have suggested the probability that the dividing line between animate and inanimate matter is less sharp than it has hitherto been regarded, so that the difficulty of finding an inclusive definition is correspondingly increased.

As a mere word, "life" is interesting in the fact that it is one of those abstract terms which has no direct antithesis; although probably most persons would regard "death" in that light. A little consideration will show that this is not the case. "Death" implies the pre-existence of "life"; there are physiological grounds for regarding death as a phenomenon of life—it is the completion, the last act of life. We cannot speak of a non-living object as *possessing* death in the sense that we speak of a living object as *possessing* life. The adjective "dead" is, it is true, applied in a popular sense antithetically to objects which have never possessed life; as in the proverbial expression "as dead as a door-nail." But in the strict sense such application is not justifiable, since the use of the terms "dead" and "living" implies either in the past or in the present the possession of the recognized properties of living matter. On the other hand, the expressions *living* and *lifeless*, *animate* and *inanimate*, furnish terms which are undoubtedly antithetical. Strictly and literally, the words *animate* and *inanimate* express the presence or absence of "soul" and not infrequently we find the terms "life" and "soul" erroneously employed as if identical.

Life not Identical with Soul.

But it is hardly necessary for me to state that the remarks I have to make regarding "life" must not be taken to apply to the conception to which the word "soul" is attached. The fact that the formation of such a conception is only possible in connexion with life, and that the growth and elaboration of the conception has only been possible as the result of the most complex processes of life in the most complex of living organisms, has doubtless led to a belief in the identity of life with soul. But unless the use of the expression "soul" is extended to a degree which would deprive it of all special significance, the distinction between these terms must be strictly maintained.

Problems of Life are Problems of Matter.

For the problems of life are essentially problems of matter; we cannot conceive of life in the scientific sense as existing apart from matter. The phenomena of life are investigated, and can only be investigated, by the same methods as all other phenomena of matter, and the general results of such investigations tend to show that living beings are governed by laws identical with those which govern inanimate matter. The more we study the manifestations of life, the more we become convinced of the truth of this statement, and the less we are disposed to call in the aid of a special and unknown form of energy to explain those manifestations.

Phenomena Indicative of Life: Movement.

The most obvious manifestation of life is "spontaneous" movement. We see a man, a dog, a bird move, and we know that they are alive. We place a drop of pond water under the microscope, and see numberless particles rapidly moving within it; we affirm that it swarms with "life." We notice a small mass of clear slime changing its shape, throwing out projections of its structureless substance, creeping from one part of the field of the microscope to another. We recognize that the slime is living; we give it a name—*Amoeba limax*—the slug amoeba. We observe similar movements in individual cells of our own body; in the white corpuscles of our blood, in connective tissue cells, in growing nerve cells, in young cells everywhere. We denote the similarity between these movements and those of the amoeba by employing the descriptive term "amoeboid" for both. We regard such movements as indicative of the possession of "life"; nothing seems more justifiable than such an inference.

Similarity of Movements in Living and Non-Living Matter.

But physicists[2] show us movements of a precisely similar character in substances which no one by any stretch of imagination can regard as living; movements of oil-drops, of organic and inorganic mixtures, even of mercury globules, which are indistinguishable in their character from those of the living organisms we have been studying—movements which can only be described by the same term "amoeboid," yet obviously produced as the result of purely physical and chemical reactions causing changes in surface tension of the fluids under examination.[3] It is therefore certain that such movements are not specifically "vital," that their presence does not necessarily denote "life." And when we investigate closely even such active movements as those of a vibratile cilium or a phenomenon so closely identified with life as the contraction of a muscle, we find that these present so many analogies with amoeboid movements as to render it certain that they are fundamentally of the same character and produced in much the same manner.[4] Nor can we for a moment doubt that the complex actions which are characteristic of the more highly differentiated organisms have been developed in the course of evolution from the simple movements characterizing the activity of undifferentiated protoplasm—movements which can themselves, as we have seen, be perfectly imitated by non-living material. The chain of evidence regarding this particular manifestation of life—movement—is complete. Whether exhibited as the amoeboid movement of the proteus animalcule or of the white corpuscle of our blood, as the ciliary motion of the infusorian or of the ciliated cell, as the contraction of a muscle under the governance of the will, or as the throbbing of the human heart responsive to every emotion of the mind, we cannot but conclude that it is alike subject

[1] *La vie et la mort*, English translation by W. J. Greenstreet, 1911, p. 54.

[2] G. Quincke, *Annal. d. Physik u. Chem.*, 1870 and 1888.

[3] The causation not only of movements but of various other manifestations of life by alterations in surface tension of living substance is ably dealt with by A. B. Macallum in a recent article in Asher and Spiro's *Ergebnisse der Physiologie*, 1911. Macallum has described an accumulation of potassium salts at the more active surfaces of the protoplasm of many cells, and correlates this with the production of cell activity by the effect of such accumulation upon the surface tension. The literature of the subject will be found in this article.

[4] G. F. Fitzgerald (*Brit. Assoc. Reports*, 1898, and *Scient. Trans. Roy. Dublin Society*, 1898) arrived at this conclusion with regard to muscle from purely physical considerations.

to and produced in conformity with the general laws of matter, by agencies resembling those which cause movements in lifeless material.[5]

It will perhaps be contended that the resemblances between the movements of living and non-living matter may be only superficial, and that the conclusion regarding their identity to which we are led will be dissipated when we endeavour to penetrate more deeply into the working of living substance. For can we not recognize along with the possession of movement the presence of other phenomena which are equally characteristic of life and with which non-living material is not endowed?

Assimilation and Disassimilation.

Prominent among the characteristic phenomena of life are the processes of assimilation and disassimilation, the taking in of food and its elaboration.[6] These surely, it may be thought, are not shared by matter which is not endowed with life. Unfortunately for this argument, similar processes occur characteristically in situations which no one would think of associating with the presence of life. A striking example of this is afforded by the osmotic phenomena presented by solutions separated from one another by semipermeable membranes or films, a condition which is precisely that which is constantly found in living matter.[7]

Chemical Phenomena Accompanying Life.

It is not so long ago that the chemistry of organic matter was thought to be entirely different from that of inorganic substances. But the line between inorganic and organic chemistry, which up to the middle of the last century appeared sharp, subsequently became misty and has now disappeared. Similarly the chemistry of living organisms, which is now a recognized branch of organic chemistry, but used to be considered as so much outside the domain of the chemist that it could only be dealt with by those whose special business it was to study "vital" processes, is passing every day more out of the hands of the biologist and into those of the pure chemist.

Identity of Physical and Chemical Processes in Living and Non-Living Matter.

Somewhat more than half a century ago Thomas Graham published his epoch-making observations relating to the properties of matter in the colloidal state: observations which are proving all-important in assisting our comprehension of the properties of living substance. For it is becoming every day more apparent that the chemistry and physics of the living organism are essentially the chemistry and physics of nitrogenous colloids. Living substance or protoplasm always, in fact, takes the form of a colloidal solution. In this solution the colloids are associated with crystalloids (electrolytes), which are either free in the solution or attached to the molecules of the colloids. Surrounding and enclosing the living substance thus constituted of both colloid and crystalloid material is a film, probably also formed of colloid, but which may have a lipoid substratum associated with it (Overton). This film serves the purpose of an osmotic membrane, permitting of exchanges by diffusion between the colloidal solution constituting the protoplasm and the circumambient medium in which it lives. Other similar films or membranes occur in the interior of protoplasm. These films have in many cases specific characters, both physical and chemical, thus favouring the diffusion of special kinds of material into and out of the protoplasm, and from one part of the protoplasm to another. It is the changes produced under these physical conditions, associated with those caused by active chemical agents formed within protoplasm and known as *enzymes*, that effect assimilation and disassimilation. Quite similar changes can be produced outside the body (*in vitro*) by the employment of methods of a purely physical and chemical nature. It is true that we are not yet familiar with all the intermediate stages of transformation of the materials which are taken in by a living body into the materials which are given out from it. But since the initial processes and the final results are the same as they would be on the assumption that the changes are brought about in conformity with the known laws of chemistry and physics, we may fairly conclude that all changes in living substance are brought about by ordinary chemical and physical forces.

Similarity of Processes of Growth and Reproduction in Living and Non-Living Matter.

Should it be contended that growth and reproduction are properties possessed only by living bodies, and constitute a test by which we may differentiate between life and non-life, between the animate and inanimate creation, it must be replied that no contention can be more fallacious. Inorganic crystals grow and multiply, and reproduce their like, given a supply of the requisite pabulum. In most cases for each kind of crystal there is, as with living organisms, a limit of growth which is not exceeded, and further increase of the crystalline matter results, not in further increase in size, but in multiplication of similar crystals. Leduc has shown that the growth and division of artificial colloids of an inorganic nature, when placed in an appropriate medium, present singular resemblances to the phenomena of the growth and division of living organisms. Even so complex a process as the division of a cell-nucleus by karyokinesis as a preliminary to the multiplication of the cell by division—a phenomenon which would prima facie have seemed, and has been commonly regarded as, a distinctive manifestation of the life of the cell—can be imitated with solutions of a simple inorganic salt, such as sodium chloride, containing a suspension of carbon particles; which arrange and rearrange themselves under the influence of the movements of the electrolytes in a manner indistinguishable from that adopted by the particles of chromatin in a dividing nucleus. And in the process of sexual reproduction, the researches of J. Loeb and others upon the ova of the sea-urchin have proved that we can no longer consider such an apparently vital phenomenon as the fertilization of the egg as being the result of living material brought to it by the spermatozoon, since it is possible to start the process of division of the ovum and the resulting formation of cells, and ultimately of all the tissues and organs—in short, to bring about the development of the whole body—if a simple chemical reagent is substituted for the male element in the process of fertilization. Indeed, even a mechanical or electrical stimulus may suffice to start development.

The Question of Vitalism and Vital Force.

Kurz und gut, as the Germans say, vitalism as a working hypothesis has not only had its foundations undermined, but most of the superstructure has toppled over, and if any difficulties of explanation still persist, we are justified in assuming that the cause is to be found in our imperfect knowledge of the constitution and working of living material. At the best, vitalism explains nothing, and the term "vital force" is an expression of ignorance which can bring us no further along the path of knowledge. Nor is the problem in any way advanced by substituting for the term "vitalism" "neo-vitalism," and for "vital force" "biotic energy."[8] "New presbyter is but old priest writ large."

Possibility of the Synthesis of Living Matter.

Further, in its chemical composition we are no longer compelled to consider living substance as possessing infinite complexity, as was thought to be the case when chemists first began to break up the proteins of the body into their simpler constituents. The researches of Miescher, which have been continued and elaborated by Kossel and his pupils, have acquainted us with the fact

[5] "Vital spontaneity, so readily accepted by persons ignorant of biology, is disproved by the whole history of science. Every vital manifestation is a response to a stimulus, a provoked phenomenon. It is unnecessary to say this is also the case with brute bodies, since that is precisely the foundation of the great principle of the inertia of matter. It is plain that it is also as applicable to living as to inanimate matter."—Dastre, op. cit., p. 280.

[6] The terms "assimilation" and "disassimilation" express the physical and chemical changes which occur within protoplasm as the result of the intake of nutrient material from the circumambient medium and its ultimate transformation into waste products which are passed out again into that medium; the whole cycle of these changes being embraced under the term "metabolism."

[7] Leduc (*The Mechanism of Life*, English translation by W. Deane Butcher, 1911) has given many illustrations of this statement. In the report of the meeting of 1867 in Dundee is a paper by Dr. J. D. Heaton (On Simulations of Vegetable Growths by Mineral Substances) dealing with the same class of phenomena. See also J. Hall Edwards's address to Birmingham and Midland Institute, November, 1911. The conditions of osmosis in cells have been especially studied by Hamburger (*Osmotischer Druck und Ionenlehre*, Wiesbaden, 1902-4).

[8] B. Moore, in *Recent Advances in Physiology*, 1906; Moore and Roaf, ibid.; and *Further Advances in Physiology*, 1909. Moore lays especial stress on the transformations of energy which occur in protoplasm. See on the question of vitalism Gley (*Revue Scientifique*, 1911) and D'Arcy Thompson (Address to Section D at Portsmouth, 1911).

that a body so important for the nutritive and reproductive functions of the cell as the nucleus—which may be said, indeed, to represent the quintessence of cell life—possesses a chemical constitution of no very great complexity, so that we may even hope some day to see the material which composes it prepared synthetically. And when we consider that the nucleus is not only itself formed of living substance, but is capable of causing other living substance to be built up—is, in fact, the directing agent in all the principal chemical changes which take place within the living cell, it must be admitted that we are a long step forward in our knowledge of the chemical basis of life. That it is the *form* of nuclear matter rather than its chemical and molecular structure which is the important factor in nuclear activity cannot be supposed. The form of nuclei, as every microscopist knows, varies infinitely, and there are numerous living organisms in which the nuclear matter is without form, appearing simply as granules distributed in the protoplasm. Not that the form assumed and the transformations undergone by the nucleus are without importance; but it is none the less true that even in an amorphous condition the material which in the ordinary cell takes the form of a "nucleus" may, in simpler organisms which have not in the process of evolution become complete cells, fulfil functions in many respects similar to those fulfilled by the nucleus of the more differentiated organism.

A similar anticipation regarding the probability of eventual synthetic production may be made for the proteins of the cell substance. Considerable progress in this direction has, indeed, already been made by Emil Fischer, who has for many years been engaged in the task of building up the nitrogenous combinations which enter into the formation of the complex molecule of protein. It is satisfactory to know that the significance of the work both of Fischer and of Kossel in this field of biological chemistry has been recognized by the award to each of these distinguished chemists of a Nobel prize.

Chemical Constitution of Living Substance.

The elements composing living substance are few in number. Those which are constantly present are carbon, hydrogen, oxygen, and nitrogen. With these, both in nuclear matter and also, but to a less degree, in the more diffuse living material which we know as protoplasm, phosphorus is always associated. "Ohne Phosphor kein Gedank" is an accepted aphorism; "Ohne Phosphor kein Leben" is equally true. Moreover, a large proportion, rarely less than 70 per cent., of water appears essential for any manifestation of life, although not in all cases necessary for its continuance, since organisms are known which will bear the loss of the greater part if not the whole of the water they contain without permanent impairment of their vitality. The presence of certain inorganic salts is no less essential, chief amongst them being sodium chloride and calcium salts, magnesium, potassium, and iron. The combination of these elements into a colloidal compound represents the chemical basis of life; and when the chemist succeeds in building up this compound it will without doubt be found to exhibit the phenomena which we are in the habit of associating with the term "life."[9]

Source of Life. The Possibility of Spontaneous Generation.

The above considerations seem to point to the conclusion that the possibility of the production of life—that is, of living material—is not so remote as has been generally assumed. Since the experiments of Pasteur, few have ventured to affirm a belief in the spontaneous generation of bacteria and monads and other micro-organisms, although before his time this was by many believed to be of universal occurrence. My esteemed friend Dr. Charlton Bastian is, so far as I am aware, the only scientific man of eminence who still adheres to the old creed, and Dr. Bastian, in spite of numerous experiments and the publication of many books and papers, has not hitherto succeeded in winning over any converts to his opinion. I am myself so entirely convinced of the accuracy of the results which Pasteur obtained—are they not within the daily and hourly experience of every one who deals with the sterilization of organic solutions?—that I do not hesitate to believe, if living torulae or mycelia are exhibited to me in flasks which had been subjected to prolonged boiling after being hermetically sealed, that there has been some fallacy either in the premisses or in the carrying out of the operation. The appearance of organisms in such flasks would not furnish to my mind proof that they were the result of spontaneous generation. Assuming no fault in manipulation or fallacy in observation, I should find it simpler to believe that the germs of such organisms have resisted the effects of prolonged heat than that they became generated spontaneously. If spontaneous generation is possible, we cannot expect it to take the form of living beings which show so marked a degree of differentiation, both structural and functional, as the organisms which are described as making their appearance in these experimental flasks.[10] Nor should we expect the spontaneous generation of living substance of any kind to occur in a fluid the organic constituents of which have been so altered by heat that they can retain no sort of chemical resemblance to the organic constituents of living matter. If the formation of life—of living substance—is possible at the present day—and for my own part I see no reason to doubt it—a boiled infusion of organic matter—and still less of inorganic matter—is the last place in which to look for it. Our mistrust of such evidence as has yet been brought forward need not, however, preclude us from admitting the possibility of the formation of living from non-living substance.[11]

Life a Product of Evolution.

Setting aside, as devoid of scientific foundation, the idea of immediate supernatural intervention in the first production of life, we are not only justified in believing, but compelled to believe, that living matter must have owed its origin to causes similar in character to those which have been instrumental in producing all other forms of matter in the universe; in other words, to a process of gradual evolution.[12] But it has been customary of late amongst biologists to shelve the investigation of the mode of origin of life by evolution from non-living matter by relegating its solution to some former condition of the earth's history, when, it is assumed, opportunities were accidentally favourable for the passage of inanimate matter into animate; such opportunities, it is also assumed, having never since recurred and being never likely to recur.[13]

Various eminent scientific men have even supposed that life has not actually originated upon our globe, but has been brought to it from another planet or from another stellar system. Some of my audience may still remember the controversy that was excited when the theory of the origin of terrestrial life by the intermediation of a meteorite was propounded by Sir William Thomson in his Presidential Address at the meeting of this association in Edinburgh in 1871. To this "meteorite" theory[14] the apparently fatal objection was raised that it would take some sixty million years for a meteorite to travel from the

[9] The most recent account of the chemistry of protoplasm is that by Botazzi (Das Cytoplasma u. die Körpersäfte) in Winterstein's *Handb. d. vergl. Physiologie*, Bd. I, 1912. The literature is given in this article.

[10] It is fair to point out that Dr. Bastian suggests that the formation of ultramicroscopic living particles may precede the appearance of the microscopic organisms which he describes.—*The Origin of Life*, 1911, p. 65.

[11] The present position of the subject is succinctly stated by Dr. Chalmers Mitchell in his article on "Abiogenesis" in the *Encyclopaedia Britannica*. Dr. Mitchell adds: "It may be that in the progress of science it may yet be possible to construct living protoplasm from non-living material. The refutation of abiogenesis has no further bearing on this possibility than to make it probable that if protoplasm ultimately be formed in the laboratory, it will be by a series of steps, the earlier steps being the formation of some substance, or substances, now unknown, which are not protoplasm. Such intermediate stages may have existed in the past." And Huxley, in his Presidential Address at Liverpool in 1870, says: "But though I cannot express this conviction" (that is, of the impossibility of the occurrence of abiogenesis, as exemplified by the appearance of organisms in hermetically sealed and sterilized flasks) "too strongly, I must carefully guard myself against the supposition that I intend to suggest that no such thing as abiogenesis ever has taken place in the past or ever will take place in the future. With organic chemistry, molecular physics, and physiology yet in their infancy and every day making prodigious strides, I think it would be the height of presumption for any man to say that the conditions under which matter assumes the properties we call 'vital' may not, some day, be artificially brought together."

[12] The arguments in favour of this proposition have been arrayed by Meldola in his Herbert Spencer Lecture, 1910, pp. 16-24. Meldola leaves the question open whether such evolution has occurred only in past years or is also taking place now. He concludes that whereas certain carbon compounds have survived by reason of possessing extreme stability, others—the precursors of living matter—survived owing to the possession of extreme lability and adaptability to variable conditions of environment. A similar suggestion was previously made by Lockyer, *Inorganic Evolution*, 1900, pp. 169, 170.

[13] T. H. Huxley, Presidential Address, 1870; A. B. Macallum, "On the Origin of Life on the Globe," in *Trans. Canadian Institute*, VIII.

[14] First suggested, according to Dastre, by de Salles-Guyon (Dastre, op. cit., p. 252). The theory received the support of Helmholtz.

nearest stellar system to our earth, and it is inconceivable that any kind of life could be maintained during such a period. Even from the nearest planet 150 years would be necessary, and the heating of the meteorite in passing through our atmosphere and at its impact with the earth would, in all probability, destroy any life which might have existed within it. A cognate theory, that of *cosmic panspermia*, assumes that life may exist and may have existed indefinitely in cosmic dust in the interstellar spaces (Richter, 1865; Cohn, 1872), and may with this dust fall slowly to the earth without undergoing the heating which is experienced by a meteorite. Arrhenius,[15] who adopts this theory, states that if living germs were carried through the ether by luminous and other radiations, the time necessary for their transportation from our globe to the nearest stellar system would be only nine thousand years, and to Mars only twenty days!

But the acceptance of such theories of the arrival of life on the earth does not bring us any nearer to a conception of its actual mode of origin; on the contrary, it merely serves to banish the investigation of the question to some conveniently inaccessible corner of the universe, and leaves us in the unsatisfactory position of affirming not only that we have no knowledge as to the mode of origin of life—which is unfortunately true—but that we never can acquire such knowledge—which it is to be hoped is not true.[16] Knowing what we know, and believing what we believe, as to the part played by evolution in the development of terrestrial matter, we are, I think (without denying the possibility of the existence of life in other parts of the universe[17]), justified in regarding these cosmic theories as inherently improbable—at least in comparison with the solution of the problem which the evolutionary hypothesis offers.[18]

The Evolutionary Hypothesis as Applied to the Origin of Life.

I assume that the majority of my audience have at least a general idea of the scope of this hypothesis, the general acceptance of which has within the last sixty years altered the whole aspect, not only of biology, but of every other branch of natural science, including astronomy, geology, physics, and chemistry.[19] To those who have not this familiarity I would recommend the perusal of a little book by Professor Judd, entitled *The Coming of Evolution*, which has recently appeared as one of the Cambridge manuals. I know of no similar book in which the subject is as clearly and succinctly treated. Although the author nowhere expresses the opinion that the actual origin of life on the earth has arisen by evolution from non-living matter, it is impossible to read either this or any similar exposition in which the essential unity of the evolutionary process is insisted upon without concluding that the origin of life must have been due to the same process, this process being, without exception, continuous, and admitting of no gap at any part of its course. Looking, therefore, at the evolution of living matter by the light which is shed upon it from the study of the evolution of matter in general, we are led to regard it as having been produced, not by a sudden alteration, whether exerted by natural or supernatural agency, but by a gradual process of change from material which was lifeless, through material on the borderland between inanimate and animate, to material which has all the characteristics to which we attach the term "life." So far from expecting a sudden leap from an inorganic, or at least an unorganized, into an organic and organized condition, from an entirely inanimate substance to a completely animate state of being, should we not rather expect a gradual procession of changes from inorganic to organic matter, through stages of gradually increasing complexity until material which can be termed "living" is attained? And in place of looking for the production of fully formed living organisms in hermetically sealed flasks, should we not rather search Nature herself, under natural conditions, for evidence of the existence, either in the past or in the present, of transitional forms between living and non-living matter?

The difficulty, nay the impossibility, of obtaining evidence of such evolution from the past history of the globe is obvious. Both the hypothetical transitional material and the living material which was originally evolved from it may, as Macallum has suggested, have taken the form of diffused ultramicroscopic particles of living substance;[20] and even if they were not diffused but aggregated into masses, these masses could have been physically nothing more than colloidal watery slime which would leave no impress upon any geological formation. Myriads of years may have elapsed before some sort of skeleton in the shape of calcareous or siliceous spicules began to evolve itself, and thus enabled "life," which must already have possessed a prolonged existence, to make any sort of geological record. It follows that in attempting to pursue the evolution of living matter to its beginning in terrestrial history we can only expect to be confronted with a blank wall of nescience.

The problem would appear to be hopeless of ultimate solution if we are rigidly confined to the supposition that the evolution of life has only occurred once in the past history of the globe. But are we justified in assuming that at one period only, and as it were by a fortunate and fortuitous concomitation of substance and circumstance, living matter became evolved out of non-living matter—life became established? Is there any valid reason to conclude that at some previous period of its history our earth was more favourably circumstanced for the production of life than it is now?[21] I have vainly sought for such reason, and if none be forthcoming the conclusion forces itself upon us that the evolution of non-living into living substance has happened more than once—and we can be by no means sure that it may not be happening still.

It is true that up to the present there is no evidence of such happening; no process of transition has hitherto been observed. But, on the other hand, is it not equally true that the kind of evidence which would be of any real value in determining this question has not hitherto been looked for? We may be certain that if life is being produced from non-living substance it will be life of a far simpler character than any that has yet been observed—in material which we shall be uncertain whether to call animate or inanimate, even if we are able to detect it at all, and which we may not be able to visualize physically even after we have become convinced of its existence.[22] But we can look with the mind's eye and follow in imagination the transformation which non-living matter may have undergone and may still be undergoing to produce living substance. No principle of evolution is better founded than that insisted upon by Sir Charles Lyell, justly termed by Huxley "the greatest geologist of his time," that we must interpret the past history of our globe by the present; that we must seek for an explanation of what has happened by the study of what is happening; that, given similar circumstances, what has occurred at one time will probably occur at another. The

[15] *Worlds in the Making*, translated by H. Borns, chap. viii, p. 221, 1908.

[16] "The history of science shows how dangerous it is to brush aside mysteries—that is, unsolved problems—and to interpose the barrier placarded 'Eternal—no thoroughfare.'"—R. Meldola, Herbert Spencer Lecture, 1910.

[17] Some authorities, such as Errera, contend, with much probability, that the conditions in interstellar space are such that life, as we understand it, could not possibly exist there.

[18] As Verworn points out, such theories would equally apply to the origin of any other chemical combination, whether inorganic or organic, which is met with on our globe, so that they lead directly to absurd conclusions.—*Allgemeine Physiologie*, 1911.

[19] As Meldola insists, this general acceptance was in the first instance largely due to the writings of Herbert Spencer: "We are now prepared for evolution in every domain. . . . As in the case of most great generalizations, thought had been moving in this direction for many years. . . . Lamarck and Buffon had suggested a definite mechanism of organic development, Kant and Laplace a principle of celestial evolution, while Lyell had placed geology upon an evolutionary basis. The principle of continuity was beginning to be recognized in physical science. . . . It was Spencer who brought these independent lines of thought to a focus, and who was the first to make any systematic attempt to show that the law of development expressed in its widest and most abstract form was universally followed throughout cosmical processes, inorganic, organic, and superorganic."—Op. cit., p. 14.

[20] There still exist, in fact, forms of life which the microscope cannot show us (E. A. Minchin, Presidential Address to Quekett Club, 1911), and germs which are capable of passing through the pores of a Chamberland filter.

[21] Chalmers Mitchell (article Life, *Encycl. Brit.*, eleventh edition) writes as follows: "It has been suggested from time to time that conditions very unlike those now existing were necessary for the first appearance of life, and must be repeated if living matter is to be reconstituted artificially. No support for such a view can be derived from observations of the existing conditions of life." Cf. also J. Hall-Edwards, op. cit.

[22] "Spontaneous generation of life could only be perceptually demonstrated by filling in the long terms of a series between the complex forms of inorganic and the simplest forms of organic substance. Were this done it is quite possible that we should be unable to say (especially considering the vagueness of our definitions of life) where life began or ended."—K. Pearson, *Grammar of Science*, second edition, 1900, p. 350.

process of evolution is universal. The inorganic materials of the globe are continually undergoing transition. New chemical combinations are constantly being formed and old ones broken up; new elements are making their appearance and old elements disappearing.[23] Well may we ask ourselves why the production of living matter alone should be subject to other laws than those which have produced, and are producing, the various forms of non-living matter; why what has happened may not happen? If living matter has been evolved from lifeless in the past, we are justified in accepting the conclusion that its evolution is possible in the present and in the future. Indeed, we are not only justified in accepting this conclusion, we are forced to accept it. When or where such change from non-living to living matter may first have occurred, when or where it may have continued, when or where it may still be occurring, are problems as difficult as they are interesting, but we have no right to assume that they are insoluble.

Since living matter always contains water as its most abundant constituent, and since the first living organisms recognizable as such in the geological series were aquatic, it has generally been assumed that life must first have made its appearance in the depths of the ocean.[24] Is it, however, certain that the assumption that life originated in the sea is correct? Is not the land surface of our globe quite as likely to have been the nidus for the evolutionary transformation of non-living into living material as the waters which surround it? Within this soil almost any chemical transformation may occur. It is subjected much more than matters dissolved in sea-water to those fluctuations of moisture, temperature, electricity, and luminosity which are potent in producing chemical changes. But whether life, in the form of a simple slimy colloid, originated in the depths of the sea or on the surface of the land, it would be equally impossible for the geologist to trace its beginnings, and, were it still becoming evolved in the same situations, it would be almost as impossible for the microscopist to follow its evolution. We are therefore not likely to obtain direct evidence regarding such a transformation of non-living into living matter in Nature, even if it is occurring under our eyes.

An obvious objection to the idea that the production of living matter from non-living has happened more than once is that, had this been the case, the geological record should reveal more than one palaeontological series. This objection assumes that evolution would in every case take an exactly similar course and proceed to the same goal—an assumption which is, to say the least, improbable. If, as might well be the case, in any other palaeontological series than the one with which we are acquainted the process of evolution of living beings did not proceed beyond Protista, there would be no obvious geological evidence regarding it; such evidence would only be discoverable by a carefully directed search made with that particular object in view.[25] I would not by any means minimize the difficulties which attend the suggestion that the evolution of life may have occurred more than once, or may still be happening, but, on the other hand, it must not be ignored that those which attend the assumption that the production of life has occurred once only are equally serious. Indeed, had the idea of the possibility of a multiple evolution of living substance been first in the field, I doubt if the prevalent belief regarding a single fortuitous production of life upon the globe would have become established among biologists—so much are we liable to be influenced by the impressions we receive in scientific childhood!

[23] See On the production of elements, W. Crookes, Address to Section B, Brit. Assoc., 1886; T. Preston, *Nature*, vol. lx, p. 180; J. J. Thomson, *Phil. Mag.*, 1897, p. 311; Norman Lockyer, op. cit., 1900; G. Darwin, Pres. Addr. Brit. Assoc., 1905.

[24] For arguments in favour of the first appearance of life having been in the sea, see A. B. Macallum, the Palaeochemistry of the Ocean, *Trans. Canad. Instit.*, 1903-4.

[25] Lankester (Art. "Protozoa," *Encycl. Brit.*, tenth edition) conceives that the first protoplasm fed on the antecedent steps in its own evolution. F. J. Allen (*Brit. Assoc. Reports*, 1896) comes to the conclusion that living substance is probably constantly being produced, but that this fails to make itself evident owing to the substance being seized and assimilated by existing organisms. He believes that "in accounting for the first origin of life on this earth it is not necessary that, as Pflüger assumed, the planet should have been at a former period a glowing fire-ball." He "prefers to believe that the circumstances which support life would also favour its origin." And elsewhere: "Life is not an extraordinary phenomenon, not even an importation from some other sphere, but rather the actual outcome of circumstances on this earth."

Further Course of Evolution of Life.

Assuming the evolution of living matter to have occurred—whether once only or more frequently matters not for the moment—and in the form suggested, namely, as a mass of colloidal slime possessing the property of assimilation, and therefore of growth, reproduction would follow as a matter of course. For all material of this physical nature—fluid or semi-fluid in character—has a tendency to undergo subdivision when its bulk exceeds a certain size. The subdivision may be into equal or nearly equal parts, or it may take the form of buds. In either case every separated part would resemble the parent in chemical and physical properties, and would equally possess the property of taking in and assimilating suitable material from its liquid environment, growing in bulk and reproducing its like by subdivision. *Omne vivum e vivo.* In this way from any beginning of living material a primitive form of life would spread, and would gradually people the globe. The establishment of life being once effected, all forms of organization follow under the inevitable laws of evolution. *Ce n'est que le premier pas qui coûte.*

We can trace in imagination the segregation of a more highly phosphorized portion of the primitive living matter, which we may now consider to have become more akin to the protoplasm of organisms with which we are familiar. This more phosphorized portion might not for myriads of generations take the form of a definite nucleus, but it would be composed of material having a composition and qualities similar to those of the nucleus of a cell. Prominent among these qualities is that of catalysis—the function of effecting profound chemical changes in other material in contact with it without itself undergoing permanent change. This catalytic function may have been exercised directly by the living substance, or may have been carried on through the agency of the enzymes already mentioned, which are also of a colloid nature, but of simpler constitution than itself, and which differ from the catalytic agents employed by the chemist in the fact that they produce their effects at a relatively low temperature. In the course of evolution special enzymes would become developed for adaptation to special conditions of life, and with the appearance of these and other modifications, a process of differentiation of primitive living matter into individuals with definite specific characters gradually became established. We can conceive of the production in this way from originally undifferentiated living substance of simple differentiated organisms comparable to the lowest forms of Protista. But how long it may have taken to arrive at this stage we have no means of ascertaining. To judge from the evidence afforded by the evolution of higher organisms it would seem that a vast period of time would be necessary for even this amount of organization to establish itself.

Formation of the Nucleated Cell.

The next important phase in the process of evolution would be the segregation and moulding of the diffused or irregularly aggregated nuclear matter into a definite nucleus around which all the chemical activity of the organism will in future be centred. Whether this change were due to a slow and gradual process of segregation or of the nature of a jump, such as Nature does occasionally make, the result would be the advancement of the living organism to the condition of a complete nucleated cell; a material advance not only in organization, but—still more important—in potentiality for future development. Life is now embodied in the cell, and every living being evolved from this will itself be either a cell or a cell aggregate. *Omnis cellula e cellulâ.*

Establishment of Sexual Differences.

After the appearance of a nucleus—but how long after it is impossible to conjecture—another phenomenon appeared upon the scene in the occasional exchange of nuclear substance between cells. In this manner became established the process of sexual reproduction. Such exchange in the unicellular Protista might and may occur between any two cells forming the species, but in the multicellular Metazoa it became—like other functions—specialized in particular cells. The result of the exchange is rejuvenescence; associated with an increased tendency to subdivide and to produce new individuals. This is due to the introduction of a stimulating or catalytic chemical agent into the cell which is to be rejuvenated, as is proved by the experi-

ments of Loeb already alluded to. It is true that the chemical material introduced into the germ cell in the ordinary process of its fertilization by the sperm cell is usually accompanied by the introduction of definite morphological elements which blend with others already contained within the germ cell, and it is believed that the transmission of such morphological elements of the parental nuclei is related to the transmission of parental qualities. But we must not be blind to the possibility that these transmitted qualities may be connected with specific chemical characters of the transmitted elements; in other words, that heredity also is one of the questions the eventual solution of which we must look to the chemist to provide.

Aggregate Life.

So far we have been chiefly considering life as it is found in the simplest forms of living substance, organisms for the most part entirely microscopic and neither distinctively animal nor vegetable, which were grouped together by Haeckel as a separate kingdom of animated nature—that of Protista. But persons unfamiliar with the microscope are not in the habit of associating the term "life" with microscopic organisms, whether these take the form of cells or of minute portions of living substance which have not yet attained to that dignity. We most of us speak and think of life as it occurs in ourselves and other animals with which we are familiar; and as we find it in the plants around us. We recognize it in these by the possession of certain properties—movement, nutrition, growth, and reproduction. We are not aware by intuition, nor can we ascertain without the employment of the microscope, that we and all the higher living beings, whether animal or vegetable, are entirely formed of aggregates of nucleated cells, each microscopic and each possessing its own life. Nor could we suspect by intuition that what we term our life is not a single indivisible property, capable of being blown out with a puff like the flame of a candle; but is the aggregate of the lives of many millions of living cells of which the body is composed. It is but a short while ago that this cell constitution was discovered; it occurred within the lifetime, even within the memory, of some who are still with us. What a marvellous distance we have travelled since then in the path of knowledge of living organisms! The strides which were made in the advance of the mechanical sciences during the nineteenth century, which is generally considered to mark that century as an age of unexampled progress, are as nothing in comparison with those made in the domain of biology, and their interest is entirely dwarfed by that which is aroused by the facts relating to the phenomena of life which have accumulated within the same period. And not the least remarkable of these facts is the discovery of the cell structure of plants and animals!

Evolution of the Cell Aggregate.

Let us consider how cell aggregates came to be evolved from organisms consisting of single cells. Two methods are possible, namely: (1) the adhesion of a number of originally separate individuals; (2) the subdivision of a single individual without the products of its subdivision breaking loose from one another. No doubt this last is the manner whereby the cell aggregate was originally formed, since it is that by which it is still produced, and we know that the life-history of the individual is an epitome of that of the species. Such aggregates were in the beginning solid; the cells in contact with one another and even in continuity; subsequently a space or cavity became formed in the interior of the mass, which was thus converted into a hollow sphere. All the cells of the aggregate were at first perfectly similar in structure and in function; there was no subdivision of labour. All would take part in effecting locomotion; all would receive stimuli from outside; all would take in and digest nutrient matter, which would then be passed into the cavity of the sphere to serve as a common store of nourishment. Such organisms are still found, and constitute the lowest types of Metazoa. Later, one part of the hollow sphere became dimpled to form a cup; the cavity of the sphere became correspondingly altered in shape. With this change in structure differentiation of function between the cells covering the outside and those lining the inside of the cup made its appearance. Those on the outside subserved locomotor functions and received and transmitted from cell to cell stimuli, physical or chemical, received by the organism; while those on the inside, being freed from such functions, tended to specialize in the direction of the inception and digestion of nutrient material; which, passing from them into the cavity of the invaginated sphere, served for the nourishment of all the cells composing the organism. The further course of evolution produced many changes of form and ever-increasing complexity of the cavity thus produced by simple invagination. Some of the cell aggregates settled down to a sedentary life, becoming plant-like in appearance and to some extent in habit. Such organisms, complex in form but simple in structure, are the sponges. Their several parts are not, as in the higher Metazoa, closely interdependent: the destruction of any one part, however extensive, does not either immediately or ultimately involve death of the rest: all parts function separately, although doubtless mutually benefiting by their conjunction, if only by slow diffusion of nutrient fluid throughout the mass. There is already some differentiation in these organisms, but the absence of a nervous system prevents any general co-ordination, and the individual cells are largely independent of one another.

Our own life, like that of all the higher animals, is an *aggregate life*—the life of the whole is the life of the individual cells. The life of some of these cells can be put an end to, the rest may continue to live; this is, in fact, happening every moment of our lives. The cells which cover the surface of our body, which form the scarf-skin and the hairs and nails, are constantly dying and the dead cells are rubbed off or cut away, their place being taken by others supplied from living layers beneath. But the death of these cells does not affect the vitality of the body as a whole. They serve merely as a protection, or an ornamental covering, but are otherwise not material to our existence. On the other hand, if a few cells, such as those nerve cells under the influence of which respiration is carried on, are destroyed or injured, within a minute or two the whole living machine comes to a standstill, so that to the bystander the patient is dead; even the doctor will pronounce life to be extinct. But this pronouncement is correct only in a special sense. What has happened is that, owing to the cessation of respiration, the supply of oxygen to the tissues is cut off. And since the manifestations of life cease without this supply, the animal or patient appears to be dead. If, however, within a short period we supply the needed oxygen to the tissues requiring it, all the manifestations of life reappear.

It is only some cells which lose their vitality at the moment of so-called "general death." Many cells of the body retain their individual life under suitable circumstances long after the rest of the body is dead. Notable among these are muscle cells. MacWilliam showed that the muscle cells of the blood vessels give indications of life several days after an animal has been killed. The muscle cells of the heart in mammals have been revived and caused to beat regularly and strongly many hours after apparent death. In man this result has been obtained as many as eighteen hours after life had been pronounced extinct (Kuliabko); in animals after days had elapsed. Waller has shown that indications of life can be elicited from various tissues many hours and even days after general death. Sherrington observed the white corpuscles of the blood to be active when kept in a suitable nutrient fluid weeks after removal from the blood vessels. A French histologist, Jolly, has found that the white corpuscles of the frog, if kept in a cool place and under suitable conditions, show at the end of a year all the ordinary manifestations of life. Carrell and Burrows have observed activity and growth to continue for long periods in the isolated cells of a number of tissues and organs kept under observation in a suitable medium. Carrell has succeeded in substituting entire organs obtained after death from one animal for those of another of the same species, and has thereby opened up a field of surgical treatment the limit of which cannot yet be descried. It is a well-established fact that any part of the body can be maintained alive for hours isolated from the rest if perfused with serum (Kronecker, frog-heart) or with an oxygenated solution of salts in certain proportions (Ringer). Such revival and prolongation of the life of separated organs is an ordinary procedure in laboratories of physiology. Like all the other instances enumerated, it is based on the fact that

the individual cells of an organ have a life of their own which is largely independent, so that they will continue in suitable circumstances to live, although the rest of the body to which they belonged may be dead.

But some cells, and the organs which are formed of them, are more necessary to maintain the life of the aggregate than others, on account of the nature of the functions which have become specialized in them. This is the case with the nerve cells of the respiratory centre, since they preside over the movements which are necessary to effect oxygenation of the blood. It is also true for the cells which compose the heart, since this serves to pump oxygenated blood to all other cells of the body; without such blood most cells soon cease to live. Hence we examine respiration and heart to determine if life is present; when one or both of these are at a standstill we know that life cannot be maintained. These are not the only organs necessary for the maintenance of life, but the loss of others can be borne longer, since the functions which they subserve, although useful or even essential to the organism, can be dispensed with for a time. The life of some cells is therefore more, of others less, necessary for maintaining the life of the rest. On the other hand, the cells composing certain organs have in the course of evolution ceased to be necessary, and their continued existence may even be harmful. Wiedersheim has enumerated more than a hundred of these organs in the human body. Doubtless Nature is doing her best to get rid of them for us, and our descendants will some day have ceased to possess a vermiform appendix or a pharyngeal tonsil; until that epoch arrives we must rely for their removal on the more rapid methods of surgery!

Maintenance of the Life of the Cell Aggregate in the Higher Animals: Co-ordinating Mechanisms.

We have seen that in the simplest multicellular organisms, where one cell of the aggregate differs but little from another, the conditions for the maintenance of the life of the whole are nearly as simple as those for individual cells. But the life of a cell aggregate such as composes the bodies of the higher animals is maintained not only by the conditions for the maintenance of the life of the individual cell being kept favourable, but also by the co-ordination of the varied activities of the cells which form the aggregate. Whereas in the lowest Metazoa all cells of the aggregate are alike in structure and function and perform and share everything in common, in higher animals (and for that matter in the higher plants also) the cells have become specialized, and each is only adapted for the performance of a particular function. Thus the cells of the gastric glands are only adapted for the secretion of gastric juice, the cells of the villi for the absorption of digested matters from the intestine, the cells of the kidney for the removal of waste products and superfluous water from the blood, those of the heart for pumping blood through the vessels. Each of these cells has its individual life and performs its individual functions. But unless there were some sort of co-operation and subordination to the needs of the body generally, there would be sometimes too little, sometimes too much gastric juice secreted; sometimes too tardy, sometimes too rapid an absorption from the intestine; sometimes too little, sometimes too much blood pumped into the arteries, and so on. As the result of such lack of co-operation the life of the whole would cease to be normal, and would eventually cease to be maintained.

We have already seen what are the conditions which are favourable for the maintenance of life of the individual cell, no matter where situated. The principal condition is that it must be bathed by a nutrient fluid of suitable and constant composition. In higher animals this fluid is the lymph which bathes the tissue elements, and is itself constantly supplied with fresh nutriment and oxygen by the blood. Some tissue cells are directly bathed by blood; and in invertebrates, in which there is no special system of lymph vessels, all the tissues are thus nourished. All cells both take from and give to the blood, but not the same materials or to an equal extent. Some, such as the absorbing cells of the villi, almost exclusively give; others, such as the cells of the renal tubules, almost exclusively take. Nevertheless, the resultant of all the give and take throughout the body serves to maintain the composition of the blood constant under all circumstances. In this way the first condition of the maintenance of the life of the aggregate is fulfilled by insuring that the life of the individual cells composing it is kept normal.

The second essential condition for the maintenance of life of the cell aggregate is the co-ordination of its parts and the due regulation of their activity, so that they may work together for the benefit of the whole. In the animal body this is effected in two ways: First, through the nervous system; and secondly, by the action of specific chemical substances which are formed in certain organs and carried by the blood to other parts of the body, the cells of which they excite to activity. These substances have received the general designation of "hormones" (ὁρμάω, to stir up), a term introduced by Professor Starling. Their action, and indeed their very existence, has only been recognized of late years, although the part which they play in the physiology of animals appears to be only second in importance to that of the nervous system itself; indeed, maintenance of life may become impossible in the absence of certain of these hormones.

Part played by Nervous System in the Maintenance of Aggregate Life: Evolution of a Nervous System.

Before we consider the manner in which the nervous system serves to co-ordinate the life of the cell aggregate, let us see how it has become evolved.

The first step in the process was taken when certain of the cells of the external layer became specially sensitive to stimuli from outside, whether caused by mechanical impressions (tactile and auditory stimuli) or impressions of light and darkness (visual stimuli) or chemical impressions. The effects of such impressions were probably at first simply communicated to adjacent cells and spread from cell to cell throughout the mass. An advance was made when the more impressionable cells threw out branching feelers amongst the other cells of the organism. Such feelers would convey the effects of stimuli with greater rapidity and directness to distant parts. They may at first have been retractile, in this respect resembling the long pseudopodia of certain Rhizopoda. When they became fixed they would be potential nerve fibres and would represent the beginning of a nervous system. Even yet (as Ross Harrison has shown), in the course of development of nerve fibres, each fibre makes its first appearance as an amoeboid cell process which is at first retractile, but gradually grows into the position it is eventually to occupy and in which it will become fixed.

In the further course of evolution a certain number of these specialized cells of the external layer sank below the general surface, partly perhaps for protection, partly for better nutrition: they became nerve cells. They remained connected with the surface by a prolongation which became an afferent or sensory nerve fibre, and through its termination between the cells of the general surface continued to receive the effects of external impressions; on the other hand, they continued to transmit these impressions to other, more distant cells by their efferent prolongations. In the further course of evolution the nervous system thus laid down became differentiated into distinct *afferent*, *efferent*, and *intermediary* portions. Once established, such a nervous system, however simple, must dominate the organism, since it would furnish a mechanism whereby the individual cells would work together more effectually for the mutual benefit of the whole.

It is the development of the nervous system, although not proceeding in all classes along exactly the same lines, which is the most prominent feature of the evolution of the Metazoa. By and through it all impressions reaching the organism from the outside are translated into contraction or some other form of cell activity. Its formation has been the means of causing the complete divergence of the world of animals from the world of plants, none of which possess any trace of a nervous system. Plants react, it is true, to external impressions, and these impressions produce profound changes, and even comparatively rapid and energetic movements, in parts distant from the point of application of the stimulus—as in the well-known instance of the sensitive plant. But the impressions are in all cases propagated directly from cell to cell—not through the agency of nerve fibres; and in the absence of anything corresponding to a nervous system it is not possible to suppose that any plant can ever acquire the

least glimmer of intelligence. In animals, on the other hand, from a slight original modification of certain cells has directly proceeded in the course of evolution the elaborate structure of the nervous system, with all its varied and complex functions, which reach their culmination in the workings of the human intellect. "What a piece of work is a man! How noble in reason! How infinite in faculty! In form and moving how express and admirable! In action how like an angel! In apprehension how like a god!" But lest he be elated with his psychical achievements, let him remember that they are but the result of the acquisition by a few cells in a remote ancestor of a slightly greater tendency to react to an external stimulus, so that these cells were brought into closer touch with the outer world; while, on the other hand, by extending beyond the circumscribed area to which their neighbours remained restricted, they gradually acquired a dominating influence over the rest. These dominating cells became nerve cells, and now not only furnish the means for transmission of impressions from one part of the organism to another, but in the progress of time have become the seat of perception and conscious sensation, of the formation and association of ideas, of memory, volition, and all the manifestations of the mind!

Regulation of Movements by the Nervous System: Voluntary Movements.

The most conspicuous part played by the nervous system in the phenomena of life is that which produces and regulates the general movements of the body—movements brought about by the so-called voluntary muscles. These movements are actually the result of impressions imparted to sensory or afferent nerves at the periphery—for example, in the skin or in the several organs of special sense; the effect of these impressions may not be immediate, but can be stored for an indefinite time in certain cells of the nervous system. The regulation of movements—whether they occur instantly after reception of the peripheral impression or result after a certain lapse of time, whether they are accompanied by conscious sensation or are of a purely reflex and unconscious character—is an intricate process, and the conditions of their co-ordination are of a complex nature involving not merely the causation of contraction of certain muscles, but also the prevention of contraction of others. For our present knowledge of these conditions we are largely indebted to the researches of Professor Sherrington.

Involuntary Movements.

A less conspicuous but no less important part played by the nervous system is that by which the contractions of involuntary muscles are regulated. Under normal circumstances these are always independent of consciousness, but their regulation is brought about in much the same way as is that of the contractions of voluntary muscles—namely, as the result of impressions received at the periphery. These are transmitted by afferent fibres to the central nervous system, and from the latter other impulses are sent down, mostly along the nerves of the sympathetic or autonomic system of nerves, which either stimulate or prevent contraction of the involuntary muscles. Many involuntary muscles have a natural tendency to continuous or rhythmic contraction which is quite independent of the central nervous system; in this case the effect of impulses received from the latter is merely to increase or diminish the amount of such contraction.

Effects of Emotions.

An example of this double effect is observed in connexion with the heart, which, although it can contract regularly and rhythmically when cut off from the nervous system and even if removed from the body, is normally stimulated to increased activity by impulses coming from the central nervous system through the sympathetic, or to diminished activity by others coming through the vagus. It is due to the readiness by which the action of the heart is influenced in these opposite ways by the spread of impulses generated during the nerve storms which we term "emotions," that in the language of poetry, and even of every day, the word "heart" has become synonymous with the emotions themselves.

The involuntary muscle of the arteries has its action similarly balanced. When its contraction is increased, the size of the vessels is lessened and they deliver less blood; the parts they supply accordingly become pale in colour. On the other hand, when the contraction is diminished, the vessels enlarge and deliver more blood; the parts which they supply become correspondingly ruddy. These changes in the arteries, like the effects upon the heart, may also be produced under the influence of emotions. Thus "blushing" is a purely physiological phenomenon due to diminished action of the muscular tissue of the arteries, whilst the pallor produced by fright is caused by an increased contraction of that tissue. Apart, however, from these conspicuous effects, there is constantly proceeding a less apparent but not less important balancing action between the two sets of nerve fibres distributed to heart and blood vessels, which are influenced in one direction or another by every sensation which we experience, and even by impressions of which we may be wholly unconscious, such as those which occur during sleep or anaesthesia, or which affect our otherwise insensitive internal organs.

Regulation of Secretion by the Nervous System.

A further instance of nerve regulation is seen in secreting glands. Not all glands are thus regulated, at least, not directly, but in those which are, the effects are striking. Their regulation is of the same general nature as that exercised upon involuntary muscle, but it influences the chemical activities of the gland cells and the outpouring of secretion from them. By means of this regulation a secretion can be produced or arrested, increased or diminished. As with muscle, a suitable balance is in this way maintained, and the activity of the glands is adapted to the requirements of the organism. Most of the digestive glands are thus influenced, as are the skin glands which secrete sweat.

Regulation of Body Temperature.

And by the action of the nervous system upon the skin glands, together with its effect in increasing or diminishing the blood supply to the cutaneous blood vessels, the temperature of our blood is regulated and is kept at the point best suited for maintenance of the life and activity of the tissues.

Effects of Emotions on Secretion.

The action of the nervous system upon the secretion of glands is strikingly exemplified, as in the case of its action upon the heart and blood vessels by the effects of the emotions. Thus an emotion of one kind—such as the anticipation of food—will cause saliva to flow, "the mouth to water"; whereas an emotion of another kind, such as fear or anxiety, will stop the secretion, causing the "tongue to cleave unto the roof of the mouth," and rendering speech difficult or impossible. Such arrest of the salivary secretion also makes the swallowing of dry food difficult; advantage of this fact is taken in the "ordeal by rice," which used to be employed in the East for the detection of criminals.

Regulation by Chemical Agents: Hormones: Internal Secretions.

The activities of the cells constituting our bodies are controlled, as already mentioned, in another way than through the nervous system—namely, by chemical agents (hormones) circulating in the blood. Many of these are produced by special glandular organs, known as internally secreting glands. The ordinary secreting glands pour their secretions on the exterior of the body, or on a surface communicating with the exterior; the internally secreting glands pass the materials which they produce directly into the blood. In this fluid the hormones are carried to distant organs. Their influence upon an organ may be essential to the proper performance of its functions, or may be merely ancillary to it. In the former case removal of the internally secreting gland which produces the hormone, or its destruction by disease, may prove fatal to the organism.

Suprarenals.

This is the case with the suprarenal capsules: small glands which are adjacent to the kidneys, although having no physiological connexion with these organs. A Guy's physician, Dr. Addison, in the middle of the last century showed that a certain affection, almost always fatal, since known by his name, is associated with disease of the

suprarenal capsules. A short time after this observation a French physiologist, Brown-Séquard, found that animals from which the suprarenal capsules are removed rarely survive the operation for more than a few days. In the concluding decade of the last century interest in these bodies was revived by the discovery that they are constantly yielding to the blood a chemical agent (or hormone) which stimulates the contractions of the heart and arteries, and assists in the promotion of every action which is brought about through the sympathetic nervous system (Langley). In this manner the importance of their integrity has been explained, although we have still much to learn regarding their functions.

Thyroid.

Another instance of an internally secreting gland which is essential to life, or at least to its maintenance in a normal condition, is the thyroid. The association of imperfect development or disease of the thyroid with disorders of nutrition and inactivity of the nervous system is well ascertained. The form of idiocy known as cretinism and the affection termed myxoedema are both associated with deficiency of its secretion; somewhat similar conditions to these are produced by the surgical removal of the gland. The symptoms are alleviated or cured by the administration of its juice. On the other hand, enlargement of the thyroid, accompanied by increase of its secretion, produces symptoms of nervous excitation, and similar symptoms are caused by excessive administration of the glandular substance by the mouth From these observations it is inferred that the juice contains hormones which help to regulate the nutrition of the body and serve to stimulate the nervous system, for the higher functions of which they appear to be essential. To quote M. Gley, to whose researches we owe much of our knowledge regarding the functions of this organ: "La genèse et l'exercice des plus hautes facultés de l'homme sont conditionnés par l'action purement chimique d'un produit de sécrétion. Que les psychologues méditent ces faits!"

Parathyroids.

The case of the parathyroid glandules is still more remarkable. These organs were discovered by Sandström in 1880. They are four minute bodies, each no larger than a pin's head, imbedded in the thyroid. Small as they are, their internal secretion possesses hormones which exert a powerful influence upon the nervous system. If they are completely removed, a complex of symptoms, technically known as "tetany," is liable to occur, which is always serious and may be fatal. Like the hormones of the thyroid itself, therefore, those of the parathyroids produce effects upon the nervous system, to which they are carried by the blood; although the effects are of a different kind.

Pituitary.

Another internally secreting gland which has evoked considerable interest during the last few years is the pituitary body. This is a small structure no larger than a cobnut attached to the base of the brain. It is mainly composed of glandular cells. Its removal has been found (by most observers) to be fatal—often within two or three days. Its hypertrophy, when occurring during the general growth of the body, is attended by an undue development of the skeleton, so that the stature tends to assume gigantic proportions. When the hypertrophy occurs after growth is completed, the extremities—namely, the hands and feet, and the bones of the face—are mainly affected; hence the condition has been termed "acromegaly" (enlargement of extremities). The association of this condition with affections of the pituitary was pointed out in 1885 by a distinguished French physician, Dr. Pierre Marie. Both "giants" and "acromegalists" are almost invariably found to have an enlarged pituitary. The enlargement is generally confined to one part—the anterior lobe—and we conclude that this produces hormones which stimulate the growth of the body generally and of the skeleton in particular. The remainder of the pituitary is different in structure from the anterior lobe and has a different function. From it hormones can be extracted which, like those of the suprarenal capsule, although not exactly in the same manner, influence the contraction of the heart and arteries. Its extracts are also instrumental in promoting the secretion of certain glands. When injected into the blood they cause a free secretion of water from the kidneys and of milk from the mammary glands, neither of which organs are directly influenced (as most other glands are) through the nervous system. Doubtless under natural conditions these organs are stimulated to activity by hormones which are produced in the pituitary and which pass from this into the blood.

The internally secreting glands which have been mentioned (thyroid, parathyroid, suprarenal, pituitary) have, so far as is known, no other function than that of producing chemical substances of this character for the influencing of other organs, to which they are conveyed by the blood. It is interesting to observe that these glands are all of very small size, none being larger than a walnut, and some—the parathyroids—almost microscopic. In spite of this, they are essential to the proper maintenance of the life of the body, and the total removal of any of them by disease or operation is in most cases speedily fatal.

Pancreas.

There are, however, organs in the body yielding internal secretions to the blood in the shape of hormones, but exercising at the same times other functions. A striking instance is furnished by the pancreas, the secretion of which is the most important of the digestive juices. This—the pancreatic juice—forms the external secretion of the gland, and is poured into the intestine, where its action upon the food as it passes out from the stomach has long been recognized. It was, however, discovered in 1889 by von Mering and Minkowski that the pancreas also furnishes an internal secretion, containing a hormone which is passed from the pancreas into the blood, by which it is carried first to the liver and afterwards to the body generally. This hormone is essential to the proper utilization of carbohydrates in the organism. It is well known that the carbohydrates of the food are converted into grape sugar and circulate in this form in the blood, which always contains a certain amount; the blood conveys it to all the cells of the body, and they utilize it as fuel. If, owing to disease of the pancreas or as the result of its removal by surgical procedure, its internal secretion is not available, sugar is no longer properly utilized by the cells of the body and tends to accumulate in the blood; from the blood the excess passes off by the kidneys, producing diabetes.

Duodenum.

Another instance of an internal secretion furnished by an organ which is devoted largely to other functions is the "pro-secretin" found in the cells lining the duodenum. When the acid gastric juice comes into contact with these cells it converts the pro-secretin into "secretin." This is a hormone which is passed into the blood and circulates with that fluid. It has a specific effect on the externally secreting cells of the pancreas, and causes the rapid outpouring of pancreatic juice into the intestine. This effect is similar to that of the hormones of the pituitary body upon the cells of the kidney and mammary gland. It was discovered by Bayliss and Starling.

Internal Secretions of the Reproductive Organs.

The reproductive glands furnish in many respects the most interesting example of organs which—besides their ordinary products, the germ cells and sperm cells (ova and spermatozoa)—form hormones which circulate in the blood and effect changes in cells of distant parts of the body. It is through these hormones that the secondary sexual characters, such as the comb and tail of the cock, the mane of the lion, the horns of the stage, the beard and enlarged larynx of a man, are produced, as well as the many differences in form and structure of the body which are characteristic of the sexes. The dependence of these so-called secondary sexual characters upon the state of development of the reproductive organs has been recognized from time immemorial, but has usually been ascribed to influences produced through the nervous system, and it is only in recent years that the changes have been shown to be brought about by the agency of internal secretions and hormones, passed from the reproductive glands into the circulating blood.[26]

Chemical Nature of Hormones.

It has been possible in only one or two instances to

[26] The evidence is to be found in F. H. A. Marshall, *The Physiology of Reproduction*, 1911.

prepare and isolate the hormones of the internal secretions in a sufficient condition of purity to subject them to analysis, but enough is known about them to indicate that they are organic bodies of a not very complex nature, far simpler than proteins and even than enzymes. Those which have been studied are all dialyzable, are readily soluble in water but insoluble in alcohol, and are not destroyed by boiling. One at least—that of the medulla of the suprarenal capsule—has been prepared synthetically, and when their exact chemical nature has been somewhat better elucidated it will probably not be difficult to obtain others in the same way.

From the above it is clear that not only is a co-ordination through the nervous system necessary in order that life shall be maintained in a normal condition, but a chemical co-ordination is no less essential. These may be independent of one another; but, on the other hand, they may react upon one another. For it can be shown that the production of some at least of the hormones is under the influence of the nervous system (Biedl, Asher, Elliott); whilst, as we have seen, some of the functions of the nervous system are dependent upon hormones.

Protective Chemical Mechanisms: Toxins and Antitoxins.

Time will not permit me to refer in any but the briefest manner to the protective mechanisms which the cell aggregate has evolved for its defence against disease, especially disease produced by parasitic micro-organisms. These, which belong with few exceptions to the Protista, are without doubt the most formidable enemies which the multicellular Metazoa, to which all the higher animal organisms belong, have to contend against. To such micro-organisms are due *inter alia* all diseases which are liable to become epidemic, such as anthrax and rinderpest in cattle, distemper in dogs and cats, small-pox, scarlet fever, measles, and sleeping sickness in man. The advances of modern medicine have shown that the symptoms of these diseases—the disturbances of nutrition, the temperature, the lassitude or excitement, and other nervous disturbances—are the effects of chemical poisons (*toxins*) produced by the micro-organisms and acting deleteriously upon the tissues of the body. The tissues, on the other hand, endeavour to counteract these effects by producing other chemical substances destructive to the micro-organisms or antagonistic to their action; these are known as *antibodies*. Sometimes the protection takes the form of a subtle alteration in the living substance of the cells which renders them for a long time, or even permanently, insusceptible (immune) to the action of the poison. Sometimes certain cells of the body, such as the white corpuscles of the blood, eat the invading micro-organisms and destroy them bodily by the action of chemical agents within their protoplasm. The result of an illness thus depends upon the result of the struggle between these opposing forces—the micro-organisms on the one hand and the cells of the body on the other—both of which fight with chemical weapons. If the cells of the body do not succeed in destroying the invading organisms it is certain that the invaders will in the long run destroy them, for in this combat no quarter is given. Fortunately we have been able, by the aid of animal experimentation, to acquire some knowledge of the manner in which we are attacked by micro-organisms and of the methods which the cells of our body adopt to repel the attack, and the knowledge is now extensively utilized to assist our defence. For this purpose protective serums or antitoxins, which have been formed in the blood of other animals, are employed to supplement the action of those which our own cells produce.

Parasitic Nature of Diseases.

It is not too much to assert that the knowledge of the parasitic origin of so many diseases, and of the chemical agents which on the one hand cause, and on the other combat, their symptoms, has transformed medicine from a mere art practised empirically into a real science based upon experiment. The transformation has opened out an illimitable vista of possibilities in the direction, not only of cure, but, more important still, of prevention. It has taken place within the memory of most of us who are here present. And only last February the world was mourning the death of one of the greatest of its benefactors—a former President of this Association[27]—who, by applying this knowledge to the practice of surgery, was instrumental, even in his own lifetime, in saving more lives than were destroyed in all the bloody wars of the nineteenth century!

Senescence and Death.

The question has been debated whether, if all accidental modes of destruction of the life of the cell could be eliminated, there would remain a possibility of individual cell life, and even of aggregate cell life, continuing indefinitely; in other words, Are the phenomena of senescence and death a natural and necessary sequence to the existence of life? To most of my audience it will appear that the subject is not open to debate. But some physiologists (for example, Metchnikoff) hold that the condition of senescence is itself abnormal; that old age is a form of disease, or is due to disease, and, theoretically at least, is capable of being eliminated. We have already seen that individual cell life, such as that of the white blood corpuscles and of the cells of many tissues, can, under suitable conditions, be prolonged for days or weeks or months after general death. Unicellular organisms kept under suitable conditions of nutrition have been observed to carry on their functions normally for prolonged periods and to show no degeneration such as would accompany senescence. They give rise by division to others of the same kind, which also, under favourable conditions, continue to live, to all appearance indefinitely. But these instances, although they indicate that in the simplest forms of organization existence may be greatly extended without signs of decay, do not furnish conclusive evidence of indefinite prolongation of life. Most of the cells which constitute the body, after a period of growth and activity, sometimes more, sometimes less prolonged, eventually undergo atrophy and cease to perform satisfactorily the functions which are allotted to them. And when we consider the body as a whole we find that in every case the life of the aggregate consists of a definite cycle of changes which, after passing through the stages of growth and maturity, always leads to senescence, and finally terminates in death. The only exception is in the reproductive cells, in which the processes of maturation and fertilization result in rejuvenescence, so that instead of the usual downward change towards senescence, the fertilized ovum obtains a new lease of life, which is carried on into the new-formed organism. The latter again itself ultimately forms reproductive cells, and thus the life of the species is continued. It is only in the sense of its propagation in this way from one generation to another that we can speak of the indefinite continuance of life. We can only be immortal through our descendants!

Average Duration of Life and Possibility of its Prolongation.

The individuals of every species of animal appear to have an average duration of existence.[28] Some species are known the individuals of which live only for a few hours, whilst others survive for a hundred years.[29] In man himself the average length of life would probably be greater than the three score and ten years allotted to him by the Psalmist if we could eliminate the results of disease and accident; when these results are included it falls far short of that period. If the terms of life given in the purely mythological part of the Old Testament were credible, man would in the early stages of his history have possessed a remarkable power of resisting age and disease. But, although many here present were brought up to believe in their literal veracity, such records are no longer accepted even by the most orthodox of theologians, and the nine hundred odd years with which Adam and his immediate descendants are credited, culminating in the nine hundred and sixty-nine of Methuselah, have been relegated, with the account of Creation and the Deluge, to their proper position in literature. When we come to the Hebrew Patriarchs we notice a considerable diminution to have taken place in what the insurance offices term

[27] Lord Lister was President at Liverpool in 1896.

[28] This was regarded by Buffon as related to the period of growth, but the ratio is certainly not constant. The subject is discussed by Ray Lankester in an early work: *On Comparative Longevity in Man and Animals*, 1870.

[29] The approximately regular periods of longevity of different species of animals furnishes a strong argument against the theory that the decay of old age is an accidental phenomenon comparable with disease.

the "expectation of life." Abraham is described as having lived only to 175 years, Joseph and Joshua to 110, Moses to 120; even at that age "his eye was not dim nor his natural force abated." We cannot say that under ideal conditions all these terms are impossible; indeed, Metchnikoff is disposed to regard them as probable; for great ages are still occasionally recorded, although it is doubtful if any as considerable as these are ever substantiated. That the expectation of life was better then than now would be inferred from the apologetic tone adopted by Jacob when questioned by Pharaoh as to his age: "The days of the years of my pilgrimage are a hundred and thirty years; few and evil have the days of the years of my life been, and have not attained unto the days of the years of the life of my fathers in the days of their pilgrimage." David, to whom, before the advent of the modern statistician, we owe the idea that seventy years is to be regarded as the normal period of life,[30] is himself merely stated to have "died in a good old age." The periods recorded for the Kings show a considerable falling-off as compared with the Patriarchs; but not a few were cut off by violent deaths, and many lived lives which were not ideal. Amongst eminent Greeks and Romans few very long lives are recorded, and the same is true of historical persons in mediaeval and modern history. It is a long life that lasts much beyond eighty; three such linked together carry us far back into history. Mankind is in this respect more favoured than most mammals, although a few of these surpass the period of man's existence.[31] Strange that the brevity of human life should be a favourite theme of preacher and poet when the actual term of his "erring pilgrimage" is greater than that of most of his fellow creatures!

The End of Life.

The modern applications of the principles of preventive medicine and hygiene are no doubt operating to lengthen the average life. But even if the ravages of disease could be altogether eliminated it is certain that at any rate the fixed cells of our body must eventually grow old and ultimately cease to function; when this happens to cells which are essential to the life of the organism, general death must result. This will always remain the universal law, from which there is no escape. "All that lives must die, passing through nature to eternity."

Such natural death unaccelerated by disease—is not death by disease as unnatural as death by accident?—should be a quiet, painless phenomenon, unattended by violent change. As Dastre expresses it, "The need of death should appear at the end of life, just as the need of sleep appears at the end of the day." The change has been led gradually up to by an orderly succession of phases, and is itself the last manifestation of life. Were we all certain of a quiet passing—were we sure that there would be "no moaning of the bar when we go out to sea"—we could anticipate the coming of death after a ripe old age without apprehension. And if ever the time shall arrive when man will have learnt to regard this change as a simple physiological process, as natural as the oncoming of sleep, the approach of the fatal shears will be as generally welcomed as it is now abhorred. Such a day is still distant; we can hardly say that its dawning is visible. Let us at least hope that, in the manner depicted by Dürer in his well-known etching, the sunshine which science irradiates may eventually put to flight the melancholy which hovers, bat-like, over the termination of our lives, and which even the anticipation of a future happier existence has not hitherto succeeded in dispersing.

[30] The expectation of life of a healthy man of fifty is still reckoned at about twenty years.

[31] "Hominis ævum cæterorum animalium omnium superat præter admodum paucorum."—Francis Bacon, *Historia vitæ et mortis*, 1637.

THE diagonal braking system introduced by Argylls, Limited, acts, we are informed, equally and simultaneously on all four wheels, and not on two of them; the effect is claimed to be an even pull without "dithering" or any tendency to skid.

THE ninth International Otological Congress was held in Boston, Mass., on August 12th to 16th, under the presidency of Dr. Clarence J. Blake. The number of practitioners who attended was 410, of whom 25 were official representatives of foreign countries. The next congress will be held at Halle in Germany in 1915 under the presidency of Dr. Alfred Denker.

An Address

ON

THE INFLUENCE OF MUSCULAR EXERCISE AND OPEN AIR ON THE BODILY FUNCTIONS.

DELIVERED AT THE PHYSIOLOGICAL SECTION OF THE BRITISH ASSOCIATION.

BY

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LAST year the distinguished President of this Section raised us to the contemplation of the workings of the soul. I ask you to accompany me in the consideration of nothing higher than a stuffy room. Every one thinks that he suffers in an ill-ventilated room owing to some change in the chemical quality of the air, be it want of oxygen, or excess of carbon dioxide, the addition of some exhaled organic poison, or the destruction of some subtle property by passage of the air over steam-coils or other heating or conducting apparatus. We hear of "devitalized" or "dead" air, and of "tinned" or "potted" air of the battleship. The good effects of open-air treatment, sea and mountain air, are no less generally ascribed to the chemical purity of the air. In reality the health-giving properties are those of temperature, light, movement, and relative moisture of the surrounding atmosphere; and leaving on one side those gross chemical impurities which arise in mines and in some manufacturing processes, and the question of bacterial infection, the alterations in chemical composition of the air in buildings where people crowd together and suffer from the effects of ill-ventilation have nothing to do with the causation of these effects.

Satisfied with the maintenance of a specious standard of chemical purity, the public has acquiesced in the elevation of sky-scrapers and the sinking of cavernous places of business. Many have thus become cave-dwellers, confined for most of their waking and sleeping hours in windless places, artificially lit, monotonously warmed. The sun is cut off by the shadow of tall buildings and by smoke—the sun, the energizer of the world, the giver of all things which bring joy to the heart of man, the fitting object of worship of our forefathers.

The ventilating and heating engineer hitherto has followed a great illusion in thinking that the main objects to be attained in our dwellings and places of business are chemical purity of the air and a uniform draughtless summer temperature.

Life is the reaction of the living substance to the ceaseless play of the environment. Biotic energy arises from the transformation of those other forms of energy—heat, light, sound, etc.—which beat upon the transformer, the living substance (B. Moore). Thus, when all the avenues of sense are closed, the central nervous system is no longer aroused and consciousness lapses. The patient who lost one labyrinth by disease, and, to escape unendurable vertigo, had the other removed by operation, was quite unable to guide his movements or realize his position in the dark. Rising from bed one night, he collapsed on the floor and remained there helpless till succour arrived.

A sense organ is not stimulated unless there is a change of rate in the transference of energy; and this to be effectual must occur in most cases with considerable quickness. If a weak agent is to stimulate, its application must be abrupt (Sherrington). Thus the slow changes of barometric pressure on the body-surface originate no skin sensations, though such changes of pressure, if applied suddenly, are much above the threshold value for touch. A touch excited by constant mechanical pressure of slight intensity fades quickly below the threshold of sensation. Thus the almost unbearable discomfort which a child feels on putting on for the first time a "natural" wool vest fades away and is no longer noticed with continual wear. Thomas à Becket soon must have become oblivious to his hair

shirt, and even to its harbingers. It is not the wind which God tempers to the shorn lamb, but the skin of the lamb to the wind. The inflow of sensations keeps us active and alive and all the organs working in their appointed functions. The cutaneous sensations are of the highest importance. The salt and sand of wind-driven sea air particularly act on the skin, and through it braces the whole body. The changing play of wind, of light, cold, and warmth stimulate the activity and health of mind and body. Monotony of sedentary occupation and of an overwarm still atmosphere endured for long working hours destroys vigour and happiness, and brings about the atrophy of disuse. We hear a great deal of the degeneration of the race brought about by city life, but observation shows us that a drayman, navvy, or policeman can live in London, or other big city, strong and vigorous, and no less so than in the country. The brain worker, too, can keep himself perfectly fit if his hours of sedentary employment are not too long, and he balances these by open-air exercise. The horses stabled, worked, and fed in London are as fine as any in the world; they do not live in windless rooms heated by radiators.

The hardy men of the North were evolved to stand the vagaries of climate—cold and warmth—a starved or full belly have been their changing lot. The full belly and the warm sun have expanded them in lazy comfort; the cold and the starvation have braced them to action. Modern civilization has withdrawn many of us from the struggle with the rigors of Nature; we seek for and mostly obtain the comfort of a full belly, and expand all the time in the warm atmosphere afforded us by clothes, wind-protected dwellings, and artificial heat—particularly so in the winter, when the health of the business man deteriorates. Cold is not comfortable, neither is hunger; therefore we are led to ascribe many of our ills to exposure to cold, and seek to make ourselves strong by what is termed good living. I maintain that the bracing effect of cold is of supreme importance to health and happiness; that we become soft and flabby, and less resistant to the attacks of infecting bacteria in the winter, not because of the cold, but because of our excessive precautions to preserve ourselves from cold; that the prime cause of "cold" or "chill" is not really exposure to cold, but to the overheated and confined air of rooms, factories, and meeting-places. Seven hundred and eleven survivors were saved from the *Titanic* after hours of exposure to cold. Many were insufficiently clad, and others wet to the skin. Only one died after reaching the *Carpathia*, and he three hours after being picked up. Those who died perished from actual cooling of the body. Exposure to cold did not cause in the survivors the diseases commonly attributed to cold.

Conditions of city and factory life diminish the physical and nervous energy, and reduce many from the vigorous health and perfectness of bodily functions which a wild animal possesses to a more secure, but poorer and far less happy, form of existence. The ill-chosen diet, the monotony and sedentary nature of daily work, the windless uniformity of atmosphere, above all, the neglect of vigorous muscular exercise in the open air and exposure to the winds and light of heaven—all these, together with the difficulties in the way of living a normal sexual life, go to make the pale, undeveloped, neurotic, and joyless citizen. Nurture in unnatural surroundings, not Nature's birthmark, moulds the criminal and the wastrel. The environment of childhood and youth is at fault rather than the stock; the children who are taken away and trained to be sailors, those sent to agricultural pursuits in the Colonies, those who become soldiers, may develop a physique and bodily health and vigour in striking contrast to their brothers who become clerks, shop assistants, and compositors.

Too much stress cannot be put on the importance of muscular exercise in regard to health, beauty, and happiness. Each muscle fills with blood as it relaxes, and expels this blood on past the venous valves during contraction. Each muscle, together with the venous valves, forms a pump to the circulatory system. It is the function of the heart to deliver the blood to the capillaries, and the function of the muscles—visceral, respiratory, and skeletal—to bring it back to the heart. The circulation is contrived for a restless mobile animal; every vessel is arranged so that muscular movement furthers the flow of blood.

The pressure of the blood in the veins and arteries under the influence of gravity varies with every change of posture. The respiratory pump, too, has a profound influence on the circulation. Active exercise, such as is taken in a game of football, entails endless changes of posture, varying compressive actions—one with another struggling in the rough and tumble of the game—forcible contractions and relaxations of the muscles, and a vastly increased pulmonary ventilation; at the same time the heart's action is accelerated and augmented and the arterial supply controlled by the vasomotor system. The influence of gravity, which tends to cause the fluids of the body to sink into the lower parts, is counteracted; the liver is rhythmically squeezed like a sponge by the powerful respiratory movements, which not only pump the blood through the abdominal viscera, but thoroughly massage these organs, and kneading these with the omentum clean the peritoneal cavity and prevent constipation. At the same time the surplus food metabolic products, such as sugar and fat, stored in the liver, are consumed in the production of energy, and the organs swept with a rapid stream of blood containing other products of muscular metabolism which are necessary to the inter-relation of chemical action. The output of energy is increased very greatly; a resting man may expend 2,000 calories per diem; one bicycling hard for most of the day expended 8,000 calories, of which only 4,000 was covered by the food eaten.

Such figures show how fat is taken off from the body by exercise, for the other 4,000 calories comes from the consumption of surplus food products stored in the tissues. While resting a man breathes some 7 litres of air, and uses 300 c.cm. of oxygen per minute, against 140 litres and 3,000 c.cm. while doing very hard labour. The call of the muscles for oxygen through such waste products as lactic acid impels the formation of red corpuscles and haemoglobin. The products of muscular metabolism in other ways not yet fully defined modify the metabolism of the whole body.

Exposure to cold, cold baths, and cold winds has a like effect, accelerating the heart and increasing the heat production, the activity of the muscles, the output of energy, the pulmonary ventilation, and intake of oxygen and food. In contrast with the soft, pot-bellied, over-fed city man, the hard, wiry fisherman trained to endurance has no superfluity of fat or tissue fluid. His blood volume has a high relative value in proportion to the mass of his body. His superficial veins are confined between a taut skin and muscles, hard as in a racehorse trained to perfection. Thus the adequacy of the cutaneous circulation and loss of heat by radiation rather than by sweating is assured. His fat is of a higher melting-point, hardened by exposure to cold. In him less blood is derived to other parts, such as adipose tissue, skin, and viscera. He uses up the oxygen in the arterial blood more completely and with greater efficiency; for the output of each unit of energy his heart has to circulate much less blood (Kreogh); his blood is sent in full volume by the well-balanced activity of his vasomotor system to the moving parts. Owing to the perfect coordination of his muscles, trained to the work, and the efficient action of his skin and cutaneous circulation—the radiator of the body—he performs the work with far greater economy and less fatigue. The untrained man may obtain 12 per cent. of his energy output as work, against 30 per cent., or perhaps even 50 per cent., obtained by the trained athlete. Hence the failure and risk suffered by the city man who rushes straight from his office to climb the Alps. On the other hand, the energetic man of business or brain worker is kept by his work in a state of nervous tension. He considers alternative lines of action, but scarcely moves. He may be intensely excited, but the natural muscular response does not follow. His heart is accelerated, and his blood pressure raised, but neither muscular movements and accompanying changes of posture, nor the respiratory pump materially aid the circulation. The activity of his brain demands a rapid flow of blood, and his heart has to do the circulatory work, as he sits still or stands at his desk, against the influence of gravity. Hence a high blood pressure is maintained for long periods at a time by vasoconstriction of the arteries in the lower parts of the body and increased action of the heart; hence, perhaps, arise those degenerative changes in the circulatory system which affect some men tireless in their mental activity.

We know that the bench-worker who stands on one leg for long hours a day may suffer from degeneration and varicosity of the veins in that leg. Long continued high arterial pressure, with systolic and diastolic pressures approximately the same, entails a stretched arterial wall, and this must impede the circulation in the vaso vasorum, the flow of tissue lymph in, and nutrition of, the wall. Since his sedentary occupation reduces the metabolism and heat production of his body very greatly, the business man requires a warmer atmosphere to work in. If the atmosphere is too warm it reduces his metabolism and pulmonary ventilation still further; thus he works in a vicious circle. Exhausting work causes the consumption of certain active principles—for example, adrenalin, and the reparation of those must be from the food. To acquire certain of the rarer principles expended in the manifestation of nervous energy more food may have to be eaten by the sedentary worker than can be digested and metabolized. His digestive organs lack the kneading and massage, the rapid circulation and oxidation of foodstuffs which is given by muscular exercise. Hence arise the digestive and metabolic ailments so common to brain workers.

Mr. Robert Milne informs me that of the thousands of children which have passed through Barnardo's Homes—there are 9,000 in the homes at any one time—not one after entering the institution and passing under its regimen and the care of his father, Dr. Milne, has developed appendicitis. Daily exercise and play, adequate rest, a regular simple diet, have ensured their immunity from this infection. It pays to keep a horse healthy and efficient; it no less pays to keep men healthy. I recently investigated the case of clerks employed in a great place of business, whose working hours are from 9 a.m. to 6 p.m. on three days, and 7 a.m. to 9 p.m. on the other three days of each week, and, working such overtime, they make £1 to £2 a week; these clerks worked in a confined space—forty to fifty of them in 8,200 cubic feet, lit with thirty electric lamps, cramped for room, and overheated in warm summer days. It is not with the chemical purity of the air of such an office that fault is to be found, for fans and large openings ensured this sufficiently. These clerks suffered from their long hours of monotonous and sedentary occupation, and from the artificial light, and the windless, overwarm, and moist atmosphere. Many a girl cashier has worked from 8 to 8.30, and on Saturdays from 8 to 10, and then has had to balance her books, and leave perhaps after midnight on Sunday morning. Her office is away in the background—confined, windless, artificially lit. The Shops Act has given a little relief from these hours. What, I ask, is the use of the State spending a million a year on sanatoriums and tuberculin dispensaries, when those very conditions of work continue which lessen the immunity and increase the infection of the workers?

The jute industry in this town of Dundee is carried out almost wholly by female and boy labour. "The average wages for women are below 12s. in eight processes, and above 12s., but under 18s., for the remaining five processes." The infant mortality has been over 170 per 1,000. The Social Union of Dundee reported in 1905 that of 885 children born to 240 working mothers, no fewer than 520, or 59 per cent., died—and almost all of them were under 5 years of age. The life of these mothers was divided between the jute factory and the one-roomed tenement. Looking such conditions squarely in the face, I say it would be more humane for the State to legalize the exposure of every other newborn infant on the hillside rather than allow children to be slowly done to death. The conditions, as given in the report, contravene those rights of motherhood which the meanest wild animal can claim.

Isolation hospitals, sputum-pots, and antispitting regulations will not stamp out tuberculosis. Such means are like shutting the door of the stable when the horse has escaped. Flügge has shown that tubercle bacilli are spread by the droplets of saliva which are carried out as an invisible spray when we speak, sing, cough, sneeze. Sputum-pots cannot control this. The saliva of cases of phthisis may teem with the bacilli. The tuberculin reaction tests carried out by Hamburger and Monti in Vienna show that 94 per cent. of all children aged 11 to 14 have been infected with tubercle. In most the infection is a mere temporary indisposition. I believe that the conditions of exhausting work, and amusement in confined and overheated atmospheres, together with ill-regulated feeding, determine largely whether the infection, which almost none can escape, become serious or not. Karl Pearson suggests that the death statistics afford no proof of the utility of sanatorium or tuberculin dispensaries, for during the very years in which such treatment has been in vogue, the fall in the mortality from tuberculosis has become less relatively to the fall in general mortality. He opines that the race is gradually becoming immune to tubercle, and hence the declination in the mortality curve is becoming flattened out—that Nature is paramount as the determinant of tuberculosis, not nurture. From a statistical inquiry into the incidence of tuberculosis in husband and wife and parent and child, Pearson concludes that exposure to infection as in married couples is of little importance, while inborn immunity or diathesis is a chief determinant. Admitting the value of his critical inquiries and the importance of diathesis, I would point out that in the last few years the rush and excitement of modern city life has increased, together with the confinement of workers to sedentary occupations in artificially lit, warm, windless atmospheres. The same conditions pertain to places of amusement, eating-houses, tube railways, etc.

Central heating, gas radiators, and other contrivances are now displacing the old open fire and chimney. This change greatly improves the economical consumption of coal and the light and cleanliness of the atmosphere. But in so far as it promotes monotonous, windless, warm atmospheres, it is wholly against the health and vigour of the nation. The open fire and wide chimney ensure ventilation, the indrawing of cold outside air, streaky air—restless currents at different temperatures, which strike the sensory nerves in the skin and prevent monotony and weariness of spirit. By the old open fires we were heated with radiant heat. The air in the rooms was drawn in cool and varied in temperature. The radiator and hot-air system give us a deadly uniformly heated air—the very conditions we find most unsupportable on a close summer's day.

In Labrador and Newfoundland, Dr. Wakefield tells me, the mortality of the fisherfolk from tuberculosis is very heavy. It is generally acknowledged to be 4 per 1,000 of the population per annum, against 1.52 for England and Wales. Some of the Labrador doctors talk of 7 and even 8 per 1,000 in certain districts. The general death-rate is a low one. The fishermen fish off shore, work for many hours a day in the fishing season, and live with their families on shore in one-roomed shanties. These shanties are built of wood, the crannies are "stogged" with moss, and the windows nailed up, so that ventilation is very imperfect. They are heated by stoves and kept at a very high temperature—for example, 80° F. Outside in the winter the temperature may be 30° below freezing. The women stay inside the shanties almost all their time, and the tuberculosis rate is somewhat higher in them. The main food is white bread, tea stewed in the pot till black, fish occasionally, a little margarine, and molasses. The fish is boiled and the water thrown away. Game has become scarce in recent years; old, dark-coloured flour—spoken of with disfavour—has been replaced by white flour. In consequence of this diet beri-beri has become rife to a most serious extent, and the hospitals are full of cases. Martin Flack and I have found by our feeding experiments that rats, mice, and pigeons cannot be maintained on white bread and water, but can live on wholemeal, or on white bread in which we incorporate an extract of the sharps and bran in sufficient amount. Recent work has shown the vital importance of certain active principles present in the outer layers of wheat, rice, etc., and in milk, meat, etc., which are destroyed by heating to 120° C. A diet of white bread or polished rice and tinned food sterilized by heat is the cause of beri-beri. The metabolism is endangered by the artificial methods of treating foods now in vogue. As to the prevalency of tuberculosis in Labrador, we have to consider the intermarriage, the bad diet, the over-rigorous work of the fishermen, the overheating of and infection in the shanties. Dr. Wakefield has slept with four other travellers in a shanty with father, mother, and ten children. In some there is scarcely room on the floor to lie down. The shanties are heated with a stove on which pots boil all the time; water runs

down the windows. The patients are ignorant, and spit everywhere—on bed, floor, and walls. In the schools the heat and smell is most marked to one coming in from the outside air. In one school 50 cubic feet per child is the allowance of space. The children are eating all day long, and are kept in close, hot confinement. They suffer very badly from decay of the teeth. Whole families are swept off with tuberculosis, and the child who leaves home early may escape, while the rest of a family dies. Here, then, we have people living in the wildest and least populated of lands with the purest atmosphere suffering from all those ill results which are found in the worst city slums—tuberculosis, beri-beri, and decayed teeth.

The bad diet probably impels the people to conserve their body heat and live in the over-warm, confined atmosphere, just as our pigeons fed on white bread sit, with their feathers out, huddled together to keep each other warm.

The metabolism, circulation, respiration, and expansion of the lung are all reduced. The warm, moist atmosphere lessens the evaporation from the respiratory tract, and therefore the transudation of tissue lymph and activity of the ciliated epithelium. The unexpanded parts of the lung are not swept with blood. Everything favours a lodgement of the bacilli, and lessens the defences on which immunity depends. In the mouth, too, the immune properties of the saliva are neutralized by the continual presence of food, and the temperature of the mouth is kept at a higher level, which favours bacterial growth. Lieutenant Siem informs me that recently in Northern Norway there has been the same notable increase in tuberculosis. The old cottage fireplaces with wide chimneys have been replaced with American stoves. In olden days most of the heat went up the chimney, and the people were warmed by radiant heat. Now the room is heated to a uniform moist heat. The Norwegians nail up the windows and never open them during the winter. At Lofoten, the great fishing centre, motor boats have replaced the old open sailing and row boats. The cabin in the motor boat is very confined, covered in with water-tight deck, heated by the engine, crowded with a dozen workers. When in harbour the fishermen used to occupy ill-fitted shanties, through which the wind blew freely; now, to save rent, they sleep in the motor-boat cabins.

Here, again, we have massive infection, and the reduction of the defensive mechanisms by the influence of the warm, moist atmosphere.

The Norwegian fishermen feed on brown bread, boiled fish, salt mutton, margarine, and drink, when in money, beer and schnapps; there is no gross deficiency in diet, as in Labrador, and beri-beri does not attack them. They return home to their villages and longshore fishing when the season is over. The one new condition which is common to the two districts is confinement in stove-heated, windless atmospheres. In old days the men were crowded together, but in open boats or in draughty shanties, and had nothing but little cooking stoves.

The conditions of great cities tend to confine the worker in the office all day, and to the heated atmosphere of club, cinema show, or music-hall in the evening. The height of houses prevents the town dweller from being blown upon by the wind, and, missing the exhilarating stimulus of the cool, moving air, he repels the dull uniformity of existence by tobacco and by alcohol, or by indulgence in food, for example, sweets, which are everywhere to his hand, and by the nervous excitement of business and amusement. He works, he eats, and is amused in warm, windless atmospheres, and suffers from a feeble circulation, a shallow respiration, a disordered digestion, and a slow rate of metabolism.

Many of the employments of modern days are detestable in their long hours of confinement and monotony. Men go up and down in a lift all day, and girls in the bloom of youth are set down in tobacco stalls in underground stations, and their health and beauty there fade, while even the blow-flies are free to bask in the sun. In factories the operatives feed machines, or reproduce the same small piece of an article day after day. There is no art, or change, no pleasure in contrivance and accomplishment. The miner, the fisherman, even the sewerman, face difficulties, changing risks, and are developed as men of character and strength. Contrast the sailor with the steward on a steamer, the drayman outside with the clerk inside who checks the goods delivered at some city office, the butcher and the tailor, the seamstress and the market woman, and one sees the enormous difference which a confined occupation makes. Monotonous sedentary employment makes for unhappiness because the inherited functional needs of the human body are neglected, and education—when the outside field of interest is narrowed—intensifies the sensitivity to the bodily conditions. The sensations arising within the body—proprioceptive sensations—come to have too large a share in consciousness in comparison with exteroceptive. In place of considering the lilies how they grow, or musing on the beauty and motions of the heavenly bodies, the sedentary worker in the smoke-befouled atmosphere, with the limited activity and horizon of an office and a disturbed digestion, tends to become confined to the inward consideration of his own viscera and their motions.

Many of the educated daughters of the well-to-do are no less confined at home; they are the flotsam and jetsam cast up from the tide in which all others struggle for existence—their lives are no less monotonous than the sweated sempstress or clerk. They become filled with "vapours," and some seek excitement not in the cannon's mouth, but in breaking windows, playing with fire, and hunger strikes. The dull monotony of idle social functions, shopping, and amusement, no less than that of sedentary work and an asexual life, impels to a simulated struggle—a theatrical performance, the parts of which are studied from the historical romances of revolution. Each man, woman, and child in the world must find the wherewithal for living, food, raiment, warmth, and housing, or must die or get some other to find it for him. It seems to me as if the world is conducted as if ten men were on an island—a microcosm—and five sought for the necessaries of life, hunted for food, built shelters and fires, made clothes of skins, while the other five strung necklaces of shells, made loin-cloths of butterfly wings, gambled with knuckle-bones, drew comic pictures in the sand, or carved out of clay frightening demons, and so beguiled from the first five the larger share of their wealth. In this land of factories, while the many are confined to mean streets and wretched houses, possessing no sufficiency of baths and clean clothing, and are ill fed, they work all day long, not to fashion for themselves better houses and clothing, but to make those unnecessaries such as "the fluff" of women's apparel, and a thousand trifles which relieve the monotony of the idle and bemuse their own minds.

The discovery of radium and its disintegration as a source of energy has enabled the physicist to extend Lord Kelvin's estimate of the world's age from some thirty to a thousand million years. Arthur Keith does not hesitate to give a million of these years to man's evolution. Karl Pearson speaks of hundreds of thousands of years. The form of the human skull, the brain capacity of man, his skill as evidenced by stone implements and cave drawings of animals in action, was the same tens of thousands of years ago as now. For ages primitive man lived as a wild animal in tropical climes, discovered how to make fire, clothe himself in skins, build shelters, and so enable himself to wander over the temperate and arctic zones. Finally, in the last few score of years, he has made houses draughtless with glass windows, fitted them with stoves and radiators, and every kind of device to protect himself from cold, while he occupies himself in the sedentary pursuits and amusements of a city life. How much better, to those who know the boundless horizon of life, to be a frontiersman and enjoy the struggle, with body hardened, perfectly fit, attuned to Nature, than to be a cashier, condemned to the occupation of a sunless, windless, pay-box. The city child, however, nurtured and educated in confinement, knows not the largeness and wonders of Nature, is used to the streets with their ceaseless movement and romantic play of artificial light after dark, and does not need the commiseration of the country mouse any more than the beetle who lives in the dark and animated burrows of his heap. But while outdoor work disciplines the body of the countryman into health, the town man needs the conscious attention and acquired educated control of his life to give him any full measure of health and happiness.

Experimental evidence is strongly in favour of my argument that the chemical purity of the air is of no importance. Analyses show that the oxygen in the

worst-ventilated school-room, chapel, or theatre is never lessened by more than 1 per cent. of an atmosphere; the ventilation through chink and cranny, chimney, door and window, and the porous brick wall, suffices to prevent a greater diminution. So long as there is present a partial pressure of oxygen sufficient to change the haemoglobin of the venous blood into oxyhaemoglobin there can arise no lack of oxygen.

At sea-level the pressure of oxygen in the pulmonary alveolar air is about 100 mm. Hg. Exposed to only half this pressure the haemoglobin is more than 80 per cent. saturated with oxygen.

In noted health resorts of the Swiss mountains the barometer stands at such a height that the concentration of oxygen is far less than in the more ventilated room. On the high plateau of the Andes there are great cities: Potosi with a hundred thousand inhabitants is at 4,165 metres, and the partial pressure of oxygen there is about 13 per cent. of an atmosphere in place of 71 per cent. at sea-level; railways and mines have been worked up to altitudes of 14,000 to 15,000 ft. At Potosi girls dance half the night, and toreadors display their skill in the ring. On the slopes of the Himalayas shepherds take their flocks to altitudes of 18,000 ft. No disturbance is felt by the inhabitants or those who reach these great altitudes slowly and by easy stages. The only disability to a normal man is diminished power for severe exertion, but a greater risk arises from want of oxygen to cases of heart disease, pneumonia, and in chloroform anaesthesia at these high altitudes. The newcomer who is carried by the railway in a few hours to the top of Pike's Peak or the Andes may suffer severely from mountain sickness, especially on exertion, and the cause of this is want of oxygen. Acclimatization is brought about in a few days' time. The pulmonary ventilation increases, the bronchial tubes dilate, the circulation becomes more rapid. The increased pulmonary ventilation lowers the partial pressure of carbon dioxide in the blood and pulmonary air, and this contributes to the maintenance of an adequate partial pressure of oxygen. Haldane and Douglas say that the percentage of red corpuscles and total quantity of the haemoglobin increases, and maintain that the oxygen is actively secreted by the lung into the blood, but the C method by which their determinations have been made has not met with unqualified acceptance. If waste products, which arise from oxygen want, alter the combining power of haemoglobin, this alteration may not persist in shed blood; for these products may disappear when the blood is exposed to air. Owing to the combining power of haemoglobin the respiratory exchange and metabolism of an animal within wide limits is independent of the partial pressure of oxygen. On the other hand, the process of combustion is dependent not on the pressure but on the percentage of oxygen. Thus the aeroplanist may become seized with altitude sickness from oxygen want, while his gas engine continues to carry him to loftier heights.

The partial pressure of oxygen in a mine at a depth of 3,000 ft. is considerably higher than at sea-level, and if the percentage is reduced to 17, while the firing of fire-damp and coal dust is impossible, there need be in the alveolar air of the lungs no lower pressure of oxygen than at sea-level. Thus the simplest method of preventing explosions in coal mines is that proposed by J. Harger—namely, to ventilate them with air containing 17 per cent. of oxygen.[1] There is little doubt that all the great mine explosions have been caused by the enforcement of a high degree of chemical purity of the air. In the old days, when ventilation was bad, there were no great dust explosions. Mr. W. H. Chambers, general manager of the Cadeby mine, where the recent disastrous explosion occurred, with the authority of his great and long practical experience of fiery mines, told me that the spontaneous combustion of coal and the danger of explosion can be wholly met by adequate diminution in ventilation. The fires can be choked out while the miners can still breathe and work. The Coal Mines Regulation Act enforces that a place shall not be in a fit state for working or passing therein if the air contains either less than 19 per cent. of oxygen or more than 1¼ per cent. of carbon dioxide. A mine liable to spontaneous combustion of coal may be exempted from this regulation by order of the Secretary of State.

The regulations impel the provision of such a ventilation current that the percentage of oxygen is sufficient for the spread of dust explosions along the intake airways, with the disastrous results so frequently recorded. If the mine were ventilated with air containing 17 per cent. of oxygen in sufficient volume to keep the miners cool and fresh, not only would explosions be prevented, but the mines could be safely worked and illuminated with electricity, and miners' nystagmus prevented, for this is due to the dim light of the safety lamp. The problem possibly may be solved by purifying and cooling the return air, and mixing and circulating this with a sufficiency of fresh air.

Owing to the fact that the percentage of CO_2 is the usual test of ventilation, and that only a very few parts per 10,000 in excess of fresh air are permitted by the English Factory Acts, it is generally supposed that CO_2 is a poison, and that any considerable excess has a deleterious effect on the human body. No supposition could be further from the truth.

The percentage of CO_2 in the worst ventilated room does not rise above 0.5 per cent., or at the outside 1 per cent. It is impossible that any excess of CO_2 should enter into our bodies when we breathe such air, for whatever the percentage of CO_2 in the atmosphere may be, that in the pulmonary air is kept constant at about 5 to 6 per cent. of an atmosphere—by the action of the respiratory centre. It is the concentration of CO_2 which rules the respiratory centre, and to such purpose as to keep the concentration both in the lungs and in the blood uniform (Haldane), the only result from breathing air containing 0.5 to 1 per cent. of CO_2 is an inappreciable increase in the ventilation of the lungs. The very same thing happens when we take gentle exercise and produce more CO_2 in our bodies.

At each breath we rebreathe into our lungs the air in the nose and large air tubes (the dead-space air), and about one-third of the air which is breathed in by a man at rest in dead-space air. Thus, no man breathes in pure outside air into his lungs. When a child goes to sleep with its head partly buried under the bed-clothes, and in a cradle confined by curtains, he rebreathes the expired air to a still greater extent, and so with all animals that snuggle together for warmth's sake. Not only the newborn babe sleeping against its mother's breast, but pigs in a sty, young rabbits, rats, and mice clustered together in their nests, young chicks under the brooding hen, all alike breathe a far higher percentage than that allowed by the Factory Acts.

To rebreathe one's own breath is a natural and inevitable performance, and to breathe some of the air exhaled by another is the common lot of men who, like animals, have to crowd together and husband their heat in fighting the inclemency of the weather.

In the Albion Brewery we analysed on three different days the air of the room where the CO_2 generated in the vats is compressed and bottled as liquid carbonic acid. We found from 0.14 to 0.93 per cent. of CO_2 in the atmosphere of that room. The men who were filling the cylinders and turning the taps on and off to allow escape of air must often breathe more than this. The men engaged in this occupation worked twelve-hour shifts, having their meals in the room. Some had followed the same employment for eighteen years, and without detriment to their health. It is only when the higher concentrations of CO_2 are breathed, such as 3 to 4 per cent. of an atmosphere, that the respiration is increased, so that it is noticeable to the resting individual; but percentages over 1 per cent. diminish the power to do muscular work, for the excess of CO_2 produced by the work adds its effect to that of the excess in the air, and the difficulty of co-ordinating the breathing to the work in hand is increased.

Haldane and Priestley found that with a pressure of 2 per cent. of an atmosphere of CO_2 in the inspired air the pulmonary ventilation of a man at rest was increased 50 per cent., with 3 per cent. about 100 per cent., with 4 per cent. about 200 per cent., with 5 per cent. about 300 per cent., and with 6 per cent. about 500 per cent. With the last, panting is severe, while with 3 per cent. it is unnoticed until muscular work is done, when the panting is increased 100 per cent. more than usual. With more than 6 per cent. the distress is very great, and headache, flushing, and sweating occur.

Divers who work in diving dress and men who work in compressed air caissons constantly do heavy and continuous labour in concentrations of CO_2 higher than 1 per cent. of an atmosphere, and so long as the CO_2 is kept below 2 to 3 per cent. they are capable of carrying out efficient work.

In the case of workers in compressed air it is important to bear in mind that the effect of the CO_2 on the breathing depends on the partial pressure and not on the percentage of this gas in the air breathed.

By a series of observations made on rats confined in cages fitted with small, ill-ventilated sleeping chambers, we have found that the temperature and humidity of the air—not the percentage of carbon dioxide or oxygen—determines whether the animals stay inside the sleeping room or come outside. When the air is cold they like to stay inside, even when the carbon dioxide rises to 4 to 5 per cent. of an atmosphere. When the sleeping chamber is made too hot and moist they come outside.

The sanitarian says it is necessary to keep the CO_2 below 0.01 per cent., so that the organic poisons may not collect to a harmful extent. The evil smell of crowded rooms is accepted as unequivocal evidence of the existence of such. He pays much attention to this and little or none to the heat and moisture of the air. The smell arises from the secretions of the skin, soiled clothes, etc. The smell is only sensed by and excites disgust in one who comes to it from the outside air. He who is inside and helps to make the "fugg" is both wholly unaware of and unaffected by it. Flügge points out, with justice, that while we naturally avoid any smell that excites disgust and puts us off our appetite, yet the offensive quality of the smell does not prove its poisonous nature. For the smell of the trade or food of one man may be horrible and loathsome to another not used to such.

The sight of a slaughterer and the smell of dead meat may be loathly to the sensitive poet, but the slaughterer is none the less healthy. The clang and jar of an engineer's workshop may be unendurable to a highly strung artist or author, but the artificers miss the stoppage of the noisy clatter. The stench of glue-works, fried-fish shops, soap and bone-manure works, middens, sewers, become as nothing to those engaged in such, and the lives of the workers are in no wise shortened by the stench they endure. The nose ceases to respond to the uniformity of the impulse, and the stench clearly does not betoken in any of these cases the existence of a chemical organic poison. On descending into a sewer, after the first ten minutes the nose ceases to smell the stench; the air therein is usually found to be far freer from bacteria than the air in a school-room or tenement.

If we turn to foodstuffs we recognize that the smell of alcohol and of Stilton or Camembert cheese is horrible to a child, while the smell of putrid fish—the meal of the Siberian native—excites no less disgust in an epicure, who welcomes the cheese. Among the hardiest and healthiest of men are the North Sea fishermen, who sleep in the cabins of trawlers reeking with fish and oil, and for the sake of warmth shut themselves up until the lamp may go out from want of oxygen. The stench of such surroundings may effectually put the sensitive, untrained brain worker off his appetite, but the robust health of the fisherman proves that this effect is nervous in origin, and not due to a chemical organic poison in the air.

Ventilation cannot get rid of the source of a smell, while it may easily distribute the evil smell through a house. As Pettenkofer says, if there is a dung-heap in a room, it must be removed. It is no good trying to blow away the smell.

Flügge and his school bring convincing evidence to show that a stuffy atmosphere is stuffy owing to heat stagnation, and that the smell has nothing to do with the origin of the discomfort felt by those who endure it. The inhabitants of reeking hovels in the country do not suffer from chronic ill health, unless want of nourishment, open-air exercise, or sleep come into play. Town workers who take no exercise in the fresh air are pale, anaemic, listless. Sheltered by houses they are far less exposed to winds, and live day and night in a warm, confined atmosphere.

The widespread belief in the presence of organic poisons in the expired air is mainly based on the statements of Brown-Séquard and d'Arsonval, statements wholly unsubstantiated by the most trustworthy workers in Europe and America. These statements have done very great mischief to the cause of hygiene, for they led ventilating engineers and the public to seek after chemical purity, and neglect the attainment of adequate coolness and movement of the air. It was stated that the condensation water obtained from expired air is poisonous when injected into animals. The evidence on which this statement is based is not only not worthy of credence, but is absurd—for example, condensation water has been injected into a mouse in a quantity equivalent to injecting 5 kilograms into a man weighing 60 kilograms. No proper controls were carried out. It is recognized now that any distilled water contaminated by bacterial products may have a toxic effect. Flack and I have for fourteen weeks kept guinea-pigs and rats confined together in a box and poorly ventilated, so that they breathed air containing 0.5 to 1.0 per cent. of CO_2. The guinea-pigs proved wholly free from anaphylactic shock on injecting rat's serum. Therefore they were not sensitized by breathing the exhaled breath of the rats for many weeks, and we are certain that no foreign protein substance is absorbed in this way. It has been proved by others, and by us, that animals so confined do well so long as they are well fed and their cages kept clean, light, cool, and dry. It is wholly untrue that they are poisoned by breathing each other's breath. The only danger arises from droplet contagion in cases of infective disease.

To study the relative effect of the temperature and chemical purity of the atmosphere I constructed a small experimental chamber of wood fitted with large glass observation windows and rendered air-tight.

On one side of the chamber were fixed two small electric heaters, and a tin containing water was placed on these in order to saturate the air with water vapour. On another side of the chamber was placed a large radiator, through which cold water could be circulated when required, so as to cool the chamber. In the roof were fixed three electric fans, one big and two small, by means of which the air of the chamber could be stirred. The chamber held approximately 3 cm. of air. In one class of experiments we shut within the chamber seven or eight students for about half an hour, and observed the effect of the confined atmosphere upon them. We kept them until the CO_2 reached 3 to 4 per cent., and the oxygen had fallen from 17 to 16 per cent. The wet-bulb temperature rose meanwhile to about 80° to 85° F., and the dry bulb a degree or two higher. The students went in chatting and laughing, but by-and-by, as the temperature rose, they ceased to talk and their faces became flushed and moist. To relieve the monotony of the experiment we have watched them trying to light a cigarette, and, puzzled by their matches going out, borrowing others only in vain. They had not sensed the diminution of oxygen, which fell below 17 per cent. Their breathing was deepened by the high percentage of CO_2, but no headache occurred in any of them from the short exposure. Their discomfort was relieved to an astonishing extent by putting on the electric fans placed in the roof. Whilst the air was kept stirred the students were not affected by the oppressive atmosphere. They begged for the fans to be put on when they were cut off. The same old stale air containing 3 to 4 per cent. CO_2 and 16 to 17 per cent. O_2 was whirled, but the movement of the air gave relief, because the air was 80° to 85° F. (wet bulb), while the air enmeshed in their clothes in contact with their skin was 98° to 99° F. (wet bulb). If we outside breathed through a tube the air in the chamber we felt none of the discomfort which was being experienced by those shut up inside. Similarly, if one of those in the chamber breathed through a tube the pure air outside he was not relieved.

R. A. Rowlands and H. B. Walker carried out a large number of observations in the chamber, each acting as subject in turn.

They recorded the effect on the respiratory ventilation and on the pulse-rate both when resting and when working. The work consisted in pulling a 20-kilogram weight about 1 metre high by means of a pulley and rope.

In some of the experiments the exhaled carbonic acid was absorbed, and in others carbonic acid was put into the chamber. The subjects inside could not tell when the gas was introduced, not even if the percentage were suddenly raised by 2. The introduction of this amount of the gas made no sensible difference to them, but increased their pulmonary ventilation.

In every one of the experiments they suffered from the heat, and the putting on of the fans gave great relief, and in particular diminished the pulse-rate during and after the working periods. The relief became much greater

when cold water was circulated through the radiator and the temperature of the chamber lowered 10° F.

The subjects wore only a vest, pants, and shoes in most of these experiments. When they wore their ordinary clothing the effect on the frequency of the pulse was more marked and the discomfort from heat and moisture much greater.

I have made observations on men dressed in the Fleuss rescue apparatus for use in mines, and exposed in a chamber to 120° F. dry bulb and 95° F. wet bulb. The skin temperature rises to the rectal temperature and the pulse is greatly accelerated—for example, to 150—and there arises danger of heat-stroke. The conditions are greatly relieved by interposing on the inspiratory tube of the apparatus a cooler filled with carbonic acid snow. The cool inspired air lowers the frequency of the heart and makes it possible for the men to do some work at 95° F. wet bulb, and to endure this temperature for two hours.

The observations made by Pembrey and Collis on the weaving-mill operatives at Darwen show that the skin of the face may be 4° to 13° F. higher in the mill when the wet bulb is 71° F. than at home when the wet bulb temperature is about 55° F. The tendency of the warm, humid atmosphere of the mill is to establish a more uniform temperature of the body as a whole (surface and deep temperatures), and to throw a tax upon the power of accommodation as indicated by the rapid pulse and low blood pressure.

The mill workers are wet with the steam blown into the sheds, their clothes and bodies are moist, and the long hours of exposure to such uncomfortable conditions are most deleterious to physical vigour and happiness. The operatives asked that they might be allowed to work without steam injectors and with diminished ventilation, so that the mill rooms became saturated with moisture evaporated from the bodies of the operatives. The old regulations, while forbidding more than 6 parts in 10,000 CO_2, put no limit to the wet-bulb temperature, and this often became excessive on hot summer days. The operatives were quite right. Less ventilation and a lower wet bulb is far better than ample ventilation and a high wet bulb. The permissible limit of CO_2 has now been raised to 11 parts in 10,000, and the wet-bulb temperature is to be controlled within reasonable limits.

The efficiency of workers in mills, mines, tunnels, stoke-holes, etc., is vastly increased by the provision of a sufficient draught of cool and relatively dry air, so as to prevent overtaxing of the heat-regulating mechanism. Mr. F. Green informs me that by means of forced draught the stoke-hole of an Orient steamer is rendered the coolest place when the ship is in the tropics.

The electric fan has vastly improved the conditions of the worker in the tropics. I would suggest that each clerk should have a fan just as much as a lamp on his desk. It will pay the employer to supply fans.

In the modern battleship men are confined very largely to places artificially lit and ventilated by air driven in by fans through ventilating shafts. The heat and moisture derived from the bodies of the men, from the engines, from cooking ranges, etc., lead to a high degree of relative moisture, and thus all parts of the ironwork inside are coated with granulated cork to hold the condensed moisture and prevent dripping.

The air smells with the manifold smells of oil, cooking, human bodies, etc., and the fresh air driven in by fans through the metal conduits takes up the smell of these, and is spoken of by the officers with disparagement as "tinned" or "potted" air. This air is heated when required by being made to pass over radiators. Many of the officers' cabins and offices for clerks, typewriters, etc., in the centre of a battleship have no portholes, and are only lit and ventilated by artificial means. The steel nature of the structure prevents the diffusion of air which takes place so freely through the brick walls of a house. The men in their sleeping quarters are very closely confined, and as the openings of the air conduits are placed in the roof between the hammocks, the men next to such openings receive a cold draught, and are likely to shut the openings. To sleep in a warm moist "fugg" would not much matter if the men were actively engaged for many hours of the day on deck and there exposed to the open air and the rigours of sea and weather. In the modern warship most of the crew work for many hours under deck, and some of the men may scarcely come on deck for weeks or even months. Considering the conditions which pertain, it seems to be of the utmost importance that all the men in a battleship should be inspected at short intervals by the medical officers so that cases of tuberculosis may be weeded out in their incipiency. The men of every rating should do deck drill for some part of every day. In the Norwegian navy every man, cooks and all, must do gymnastic drill on deck once a day. In the case of our navy, with voluntary service, the men should welcome this in their own interest.

In a destroyer visited by me twelve men occupied quarters containing about 1,700 cubic feet of air. There was a stove with iron pipe for chimney, from which fumes of combustion must leak when in use, and a fan which would not work. When the men are shut down the moisture is such that boots, etc., go mouldy, and the water drips off the structure. The cooling effect of the sea-water washing over the steel shell of the boat is beneficial in keeping down the temperature in these confined and ill-ventilated quarters. On the manœuvring platform in the engine-room the wet-bulb temperature reaches a very high degree owing to the slight escape of steam round the turbines. Commander Domvile was kind enough to send me the wet and dry bulb temperatures taken there on a number of days. The wet bulb was found to be never below 80° F., sometimes reached 95° and even 98° F. It is impossible for officers to work at these temperatures without straining the heat-regulating mechanism of the body and diminishing their health and working capacity. If such wet-bulb temperatures are unavoidable, means should be provided, such as fans, which would alleviate the discomfort and fatigue caused thereby. A supply of compressed air fitted with a nozzle might be arranged and used occasionally to douche the body with cool air. I have tried this plan and found it very effectual, and can recommend the compressed-air bath as the substitute for a bracing cold wind.

The suitability of the clothing is of the greatest importance, not only to the comfort but to the efficiency of man as a working machine—for example, power of soldiers to march. On a still day the body is confined by the clothes as if by a chamber of stagnant air, for the air is enclosed in the meshes of the clothes and the layer in contact with the skin becomes heated to body temperature and saturated with moisture.

The observations of Pembrey show that himself and four soldiers, marching in drill order on a moderately warm day, lost more water and retained more water in their clothes than on another similar day when they worked with no jacket on. The average figures were: Loss of moisture 1,600, against 1,200 grams, and water retained in clothes 254, against 109 grams. With no jacket the pulse was, on the average, increased 28 against 41 in drill order, and rectal temperature 1° against 1.5° F. The taking off of the jacket or throwing open of the jacket and vest very greatly increase the physiological economy of a march. It is absurd that on a hot summer day Boy Scouts should march with a coloured scarf knotted round their necks. Nothing should be worn for ornament or smartness which increases the difficulty of keeping down the body temperature. The power to march and the efficiency of an army depend on prevention of heart stagnation and avoidance of fatigue of the heart.

I conclude, then, that all the efforts of the heating and ventilating engineer should be directed towards cooling the air in crowded places and cooling the bodies of the people by setting the air in motion by means of fans. In a crowded room the air confined between the bodies and clothes of the people is almost warmed up to body temperature and saturated with moisture, so that cooling of the body by radiation, convection, and evaporation becomes reduced to a minimum. The strain on the heat-regulating mechanism tells on the heart. The pulse is accelerated, the blood is sent in increased volume to the skin, and circulates there in far greater volume, while less goes through the viscera and brain. As the surface temperature rises, the cutaneous vessels dilate, the veins become filled, the arteries may become small in volume, and the blood pressure low, the heart is fatigued by the extra work thrown upon it. The influence of the heat stagnation is shown by the great acceleration of the pulse when work is

done and the slower rate at which the pulse returns to its former rate on resting.

The increased percentage of carbonic acid and diminution of oxygen which has been found to exist in badly ventilated churches, schools, theatres, barracks, is such that it can have no effect upon the incidence of respiratory disease and higher death-rate, which statistical evidence has shown to exist among persons living in crowded and unventilated rooms. The conditions of temperature, moisture, and windless atmosphere in such places primarily diminishes the heat loss, and secondarily the heat production—that is, the activity of the occupants, together with total volume of air breathed, oxygen taken in and food eaten. The whole metabolism of the body is thus run at a lower plane, and the nervous system and tone of the body is unstimulated by the monotonous, warm, and motionless air. If hard work has to be done it is done under conditions of strain. The number of pathogenic organisms is increased in such places, and these two conditions run together—diminished immunity and increased mass influence of infecting bacteria.

The volume of blood passing through, and of water vapour evaporated from, the respiratory mucous membrane must have a great influence on the mechanisms which protect this tract from bacterial infection. While too wet an atmosphere lessens evaporation, a hot dry atmosphere dries up the mucous membrane. As the immunizing powers depend on the passage of blood plasma into the tissue spaces, it is clear that a proper degree of moisture is important. The temperature, too, must have a great influence on the scavenger activity of the ciliated epithelium and leucocytes in the mucous membrane of the nose.

In the warm, moist atmosphere of a crowded place the infection from spray sneezed coughed, or spoken out is enormous. On passing out from such an atmosphere into cold moist air the respiratory mucous membrane of the nose is suddenly chilled, the blood vessels constricted, and the defensive mechanism of cilia and leucocyte checked. Hence the prevalence of colds in the winter. In the summer the infection is far less. We are far more exposed to moving air, and the sudden transition from a warm to a cold atmosphere does not occur. We believe that infection is largely determined by (1) the mass influence of the infecting agent; (2) the shallow breathing and diminished evaporation from, and flow of tissue lymph through, the respiratory tract, in warm, moist confined air. Colds are not caught by exposure to cold *per se*, as is shown by the experience of arctic explorers, sailors, shipwrecked passengers, etc.

We have very great inherent powers of withstanding exposure to cold. The bodily mechanisms become trained and set to maintain the body heat by habitual exposure to open-air life. The risk lies in overheating our dwellings and overclothing our bodies, so that the mechanisms engaged in resisting infection become enfeebled, and no longer able to meet the sudden transition from the warm atmosphere of our rooms to the chill outside air of winter. The dark and gloomy days of winter confine us within doors, and, by reducing our activity and exposure to open air, depress the metabolism; the influence of smoke and fog, gloom of house and streets, cavernous places of business and dark dwellings, intensify the depression. The immunity to a cold after an infection lasts but a short while, and when children return, after the summer holidays, to school and damp, chill autumn days, infection runs around. The history of hospital gangrene and its abolition by the aseptic methods of Lister—likewise the history of insect-borne disease—show the great importance of cleanliness in crowded and much occupied rooms. The essentials required of any good system of ventilation are, then, (1) movement, coolness, proper degree of relative moisture of the air; (2) reduction of the mass influence of pathogenic bacteria. The chemical purity of the air is of very minor importance, and will be adequately insured by attendance to the essentials.

As the prevention of spray (saliva) infection by ventilation is impossible in crowded places, it behoves us to maintain our immunity at a high level. We may seek to diminish the spray output of those infected with colds by teaching them to cough, sneeze, and talk with a handkerchief held in front of the mouth, or to stay at home until the acute stage is past.

In all these matters nurture is of the greatest importance, as well as Nature. A man is born with physical and mental capacities small or great, with inherited characteristics, with more or less immunity to certain diseases, with a tendency to longevity of life or the opposite; but his comfort and happiness in life, the small or full development of his physical and mental capacities, his immunity, and his longevity of life are undoubtedly determined to a vast extent by nurture.

By nurture—use the word in its widest sense to include all the defensive methods of sanitary science—plague, yellow fever, malaria, sleeping sickness, cholera, hospital gangrene, etc., can be prevented by eliminating the infecting cause; small-pox and typhoid by this means, and also by vaccination; and most of the other ills which flesh is supposed to be heir to can be kept from troubling by approximating to the rules of life which a wild animal has to follow in the matter of a simple and often spare diet, hard exercise, and exposure to the open air.

There is nothing more fallacious than the supposition commonly held that over-feeding and over-coddling indoors promote health. The two together derange the natural functions of the body. He who seeks to save his life will lose it.

The body of a newborn babe is a glorious and perfect machine, the heritage of millions of years of evolution.

> Not in entire forgetfulness,
> And not in utter nakedness,
> But trailing clouds of glory do we come.
> . . . . . .
> Shades of the prison house begin to close
> Upon the growing Boy.

The ill-conditioned body, anaemic complexion, and undersized muscles, or the fat and gross habit, the decay of the teeth, the disordered digestion, the nervous irritability and unhappiness are the result of "Nurture"—not Nature.

In institutions children may be disciplined to vigorous health. After leaving school they are set adrift to face monotonous work in confined places, amusement in music-halls and cinema shows in place of manly exercise in the open air, injudicious diet, alcohol, and tobacco—everything which the trainer of an athlete would repel.

> And custom lie upon him with a weight
> Heavy as frost, and deep almost as life.

REFERENCE.
[1] *Trans. Inst. of Mining Engineers*, 1912.

THE Italian Minister of Posts and Telegraphs has appointed a commission to study the effects of the telephone service in relation to hygiene.

DR. LÉON LABBÉ, member of the French Senate, in speaking of the number of victims of professional duty in the French Army Medical Service, says that in the last eighty years 548 medical officers have been killed in battle, while 399 have died of plague, yellow fever, typhoid, and cholera. In the Crimea the French army lost 18.22 per cent. of its medical officers, while the loss of combatant officers was 14.17 per cent. In the Mexican expedition the army lost 20 per cent. of its medical officers and 4 per cent. of its combatant officers from yellow fever.

A YEAR ago, at the suggestion of the Italian Minister of Public Instruction, a commission was appointed to study the question of publishing the complete writings of Leonardo da Vinci. The first step was to obtain photographic reproductions of all the material preserved in Italy and other countries in public and private libraries. Commendatore Gino Modigliani has offered a sum of £4,000 towards the expenses of this undertaking. Leonardo was an anatomist as well as an artist and an engineer; and it is held by some that he anticipated much of the original work in anatomy which is usually attributed to Vesalius.

THE first meeting of the International Paediatric Association will be held in Paris, the opening day being fixed for October 7th. Two questions are proposed for discussion: infantile anaemias and forms of acute poliomyelitis. Each discussion will be opened by four reports, as follows:—Infantile anaemias: (*a*) digestive origin (Czerny); (*b*) chronic infective diseases (Benvendorff); (*c*) parasites of the blood (Jemma); (*d*) anaemias in general (Tixier). Acute poliomyelitis: (*a*) epidemiology (Wickmann); (*b*) pathological anatomy and experimental research (Zapper); (*c*) symptomatology (Müller); (*d*) surgical treatment (Ombrédanne).

EIGHTIETH ANNUAL MEETING

OF THE

British Medical Association.

Held in Liverpool on July 19th, 20th, 22nd, 23rd, 24th, 25th, and 26th.

PROCEEDINGS OF SECTIONS.

SECTION OF ANAESTHETICS.

DUDLEY W. BUXTON, M.D., B.S., President.

PRESIDENT'S INTRODUCTORY REMARKS.

IN welcoming both our visitors and the members of the British Medical Association to the Section of Anaesthetics it seems pertinent that I should say a few words about the work which lies before us and upon the subject of the present position of anaesthetics.

Although attempts were made in this country, within a few years of the application of ether and chloroform to the purposes of surgery, to evolve scientific method out of the chaos of empiric handicraft—and the name of John Snow must always stand pre-eminent in this connexion—yet comparatively little progress was made to this end. Alike were the methods of producing anaesthesia jejune, and the attitude of our profession was unsympathetic towards serious workers among those who specialized as anaesthetists. While facilities and rewards were provided for younger men working in the newer branches of medicine and surgery, in anaesthesia research passed into the hands of the physiologists, the surgeons, and the physicists, although, fortunately for us, it was by them ably and strenuously pushed forward. The action of the British Medical Association stands out conspicuously in this regard and merits to the full the gratitude of anaesthetists. It has been this body which from the earliest days of anaesthesia has granted money and extended encouragement to those who sought to unravel some of the secrets of our calling. You all know that committees have been formed from time to time and have been munificently financed by our Association, and every effort has been made to increase our knowledge both of the science and of the art of anaesthesia. In spite of all the research which has been undertaken, we see that, even to-day, our methods in many ways are crude, and in many instances our individual knowledge is below the standard which obtains in kindred branches of surgery. We still labour under many disabilities, while those privileges which should exist as our right are accorded only to individuals in token of recognition of personal distinction. An item in our programme—the discussion upon the advantages of an anaesthetist having facilities granted him for studying his patient before giving the anaesthetic—is germane in this connexion. It is a detriment alike to the patient and to the operator, as well as to the anaesthetist, that the latter should be divorced from personal relations with the public. The confidence of the patient gained enables the anaesthetist to bring about a more perfect anaesthesia, to avoid risks, and to adopt the best and safest choice of his method. To the failures or ineptitudes of those who administer anaesthetics without possessing an adequate grounding in the science which should always underlie the art, we owe the confusing multiplicity of methods and the evanescent uses of anaesthetics which are, alas, only too common.

We have during the next few days to discuss some recently adopted methods, such as intravenous infusion of drugs, the intratracheal insufflation of anaesthetics, and the mixed method of giving alkaloidal substances before general anaesthetics. We have also to discuss the rival plans, those which are inhalational and those called local analgesia, spinal analgesia, although these are essentially but variants of the root principle of nerve blocking or regional analgesia. All thoughtful anaesthetists must welcome each and every addition to our armamentarium, but equally must they recognize the dangers incident to the hasty adoption of whatever is new, as if novelty itself were proof of superiority. It is natural that the enthusiasm which is engendered in the discoverer of a fresh method should lead him to regard it as a panacea against the defects and dangers of all methods of inducing unconsciousness to pain. It is, however, necessary that such enthusiasm should be brushed aside and its glamour discounted when we consider the evidence for and against each fresh claimant in the anaesthetic arena. Hence is the value of such meetings as the present. We come here to learn what has been the experience of others and to criticize impartially the claims advanced. I deprecate the attitude of some persons who, without any adequate knowledge, and even without any personal experience of modern methods, express unstinted disapproval of those which have not come within their purview; indeed, this is as dangerous to the advancement of our science as its antithesis—the perfervid praise of methods the value of which has been as yet inadequately tested. In my own experience each new method sooner or later shows itself to possess some merits and too often to involve inherent drawbacks, so that, although I make a practice of trying all things, I do not anticipate the finding of perfection; indeed, it seems to me that the chiefest duties of the expert anaesthetist, after he has perfected himself in all current methods, consist in his selection not only of the drug to be used, but of the method of its application. Whether he conducts a regional and local anaesthesia, a spinal analgesia, an intravenous infusion of ether, an inhalation of ether by an open method, or the use of chloroform by a dosimetric system, such as that by the Harcourt inhaler, he is obliged to study his patient, first, as a pathological subject; secondly, from his psychological side; and, thirdly, he has to consider which method, when two or more are safe, will enable the operator to do the best work and do it with the greatest celerity and attention to detail. Technical skill in anaesthesia, as in general surgery, is of the utmost importance, but it must always stand second to diagnosis and prognosis.

By diagnosis in connexion with anaesthesia I mean the power of grasping those features in the case which call for a particular method and drug; and by prognosis I wish to connote the looking beyond the period occupied by the actual anaesthetization on to the after-effects brought about by the subtile interaction of drugs and tissues. At one time we were invited to believe that spinal and other forms of regional analgesia removed such consequential perils; alas, at present we know that the *lapis divinus* which shall transmute the base metal of our present dangerous methods into one which is freed alike from peril and distress has yet to be discovered. The experience of countries in which there has been no specialization in anaesthesia, and therefore no experts, has demonstrated the advantage which may accrue from such restricted practice. Technique, as I have said, with patience may be acquired, but the real knowledge essential for the greater problems of anaesthesia require, it seems to me, years of patience and experience and a whole-time service. I spoke of our disabilities. May I advert to one which stands in the forefront and seriously impedes the knowledge of our art? It is the lack of status suffered by the anaesthetist and the inadequacy of his rewards. Very few men commencing practice as an anaesthetist can afford to devote the time to research which should be a *sine qua non* for beginners. He obtains a hospital appointment or forms the nucleus of a *clientèle*, which latter is too often originated in a kindly wish on the part of those who desire to help a friend without any relation to the aspirant's knowledge or skill; he then embarks upon money-getting, and neglects or cannot achieve experimental research. In this respect the young anaesthetist is more hardly placed than are the medical or surgical men of his year. For them there are teaching or other posts, themselves stepping-stones to preferment in their special lines of work; for him there are none. It may be said that the specialty of anaesthetics is too narrow for its being placed in association with the wider subjects comprised in medicine. This statement cannot be accepted by those who are conversant with the subject, or who have followed the advances made during the past few decades. That these advances have not been greater is due in no small

measure to the fact that comparatively few of the ablest students will submit to the equivocal position in which an anaesthetist is often placed, nor will they devote time, energy, and money for research in a subject which is only now emerging into the goodwill of the public and shaking itself free from being a mere craft practised by those whose inclinations have been thwarted in other directions. Happily to-day the more enlightened members of our profession recognize the valuable aid which the scientific anaesthetist can give, and so extend to him the same treatment as to others who are masters of their craft. It is, then, for us anaesthetists to prove by our devotion to the scientific side of anaesthesia that we regard our specialty as one worthy of men's esteem, and this we can best do by research, patient interrogation of Nature under test conditions rather than by mere tabulation of cases, since only in this way can be eliminated the unknown factors which for ever baulk us when we confine ourselves to observations made within a few hours of giving an anaesthetic. It may be that very few can undertake this work; but to those few I say their energy and labour will meet with great reward—not, perhaps, financial success, but that better success which consists in having extended the knowledge of their calling, and, incidentally, of having trained themselves in methods of accuracy and thought, and of having widened their own outlook.

DISCUSSION ON INTRAVENOUS INFUSION ANAESTHESIA.

OPENING PAPER.

By FELIX ROOD, M.B., B.S.Durh.,
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RECENTLY considerable attention has been devoted to the question as to what extent the administration of anaesthetic drugs may be responsible for serious symptoms, or even death, occurring in patients several days after the operation. Unfortunately the symptoms which arise from an acid intoxication are identical with whose which may be the result of septic absorption, so that it is frequently impossible to say whether a given case is acid toxaemia or septic toxaemia or a combination of the two. Although unanimity upon this question is far from having been reached, one may venture to express the opinion that many anaesthetists would hesitate to administer chloroform in cases of acute or chronic septic processes in the young, in states of inanition, or in diabetes.

The increasing importance which is being attached to the possibilities of acidosis has naturally led to greater attention being directed to the use of ether, as this drug is undoubtedly less apt to produce this complication than is chloroform. It was soon found that the closed inhaler was by no means so indispensable as had been supposed. The administration of ether by the open method so long familiar in America proved to be comparatively easy and in a high degree satisfactory. A further great improvement in the technique of the open ether anaesthesia was the discovery that by the combination of the actions of certain narcotics and of atropine with that of ether an anaesthesia could be produced which possessed all the advantages of chloroform anaesthesia and none of its special dangers.

In regard to the safety of the patient during the operation, in regard to post-anaesthetic acidosis, and, finally, in regard to post-anaesthetic gastric and pulmonary complications, this method gives results which are on the whole decidedly better than those of chloroform. It is probable that this opinion would be most likely to meet with opposition in relation to post-anaesthetic lung complications. That it is not altogether unsupported by evidence is shown by the records given in the next column from some of the German surgical clinics.

These figures show an advantage on the side of ether so great as to be unmistakable. Excellent though this method of giving of ether is, there are certain inherent defects in any method of administration through the respiratory tract. Although in most cases the results obtained by inhalation are good, cases very frequently do arise in which the objections to this method are of very great importance. An attempt may, perhaps, be made

Anaesthesia Produced by Chloroform.

Surgeon.	Number of Laparotomies.	Percentage of Lung Complications.
Korte...	3,909	7.2
Czerny	1,302	3.9
Kummel	1,754	2.5
Trendelenburg	1,829	5.4
Anaesthesia Produced by Ether (Open Method).		
Krönlein	1,409	0.56

here to enumerate some of the more obvious difficulties necessarily contingent upon the use of what may be called the respiratory method. In the first case, one is limited to the use of highly-volatile substances, and it is probable that if solubility rather than volatility is the physical factor required, the number of available drugs may be greatly increased—and we know that already drugs other than ether and chloroform are being employed for this purpose.

Secondly, the respiratory method must always in essence be indirect. The anaesthetic is absorbed from the vapour introduced into the air passages, so that the dose actually entering the blood is rendered variable by the notorious variability with which it enters the chest, being subject to all the accidents of laryngeal spasm, respiratory spasm, and of variations in the depth and energy of respiration. Furthermore, even supposing the vapour to be regularly introduced, its absorption must be influenced by the variations in pressure to which it is exposed, and by the presence of mucus, the secretion of which it tends to excite.

Thirdly, the exposure of a very large surface of the respiratory tract to an irritating vapour must be regarded as a disadvantage and danger. To any one who has inhaled a breath of a strong vapour of ether it must always be a source of surprise that serious complications are not always produced by a long anaesthesia, as we know they sometimes are.

Fourthly, it is clear that to produce anaesthesia the anaesthetic must be introduced into the blood and maintained there at a certain percentage. Any further amount present in the body will act as a reservoir upon which the patient will continue to draw after the administration has been stopped. We know from experience only too well that during the course of an anaesthetic the patient may develop respiratory obstruction, and continue fatally to absorb the chloroform which is shut up in his lungs.

Fifthly, in operations upon the mouth, jaws, or pharynx, the difficulties of the surgeon, in the nature of the case, quite great enough, are much increased by his having to share the field of operation with the anaesthetist. And it not infrequently happens that the surgeon or the anaesthetist has to be content with a less degree of technical perfection than he would have attained if he had not been embarrassed by the necessities of the other.

It is clear that if the direct administration of the anaesthetic by the blood can be safely effected, it disposes of all these difficulties. The injection of anaesthetic drugs dissolved in saline was used experimentally upon animals by several observers before Burghardt introduced its use in man. His striking successes encouraged others to follow, and in April of last year, at the Berlin Surgical Congress, Kummel was able to report 90 cases. He spoke of the method with the warmest praise, and said that patients, especially the old and feeble, frequently left the table in a much better condition than they came to it; they quickly recovered from the anaesthetic, and in no case of the 90 did post-anaesthetic vomiting occur.

Technique of the Method.

Ether is soluble in saline to the extent that 100 volumes of normal saline solution dissolves 10.8 volumes of ether. This, it may be said at once, is a true solution; it is not an emulsion or suspension. So that for purposes of anaesthesia a 10 per cent. solution of ether can easily be prepared. Originally a 5 per cent. solution of ether was used, but it has been found by experience that a 6, 7, or 8 per cent. solution may be employed with safety as occasion

requires. The method originally adopted for the introduction of this ether saline was one of interrupted flow, enough of the solution to produce anaesthesia being run in, then the administration was stopped until the patient showed signs of coming round, when a further supply was introduced, and so on.

There are two very obvious disadvantages to this technique. First, the interruption of the flow brings with it a tendency for the blood to coagulate in the cannula and so to obstruct the flow of ether saline. The other objection which is inherent in any discontinuous administration is that a greater percentage of anaesthetic is introduced into the blood at certain times than is required to produce adequate anaesthesia. This principle has been so fully emphasized by those to whom we owe the introduction of the Vernon Harcourt inhaler that it is not necessary to labour it further here.

In order to get over these difficulties two changes have been made in the technique:

1. A variation in the strength of ether saline, according to the individual needs of the patient.
2. The establishment of a continuous flow of ether saline, with minute graduation of the rate and amount of the inflowing stream.

A variation in the percentage of ether used is an advantage in that it tends to reduce the total amount of fluid injected. Some of the earlier observers used a 10 per cent. solution of ether, but found that this concentration occasionally produced a transient haemoglobinuria. Speaking from personal experience, slight haemoglobinuria occurred in a few cases after the use of 10 per cent. solution, but if the percentage of ether remained below 8 per cent. no haemoglobinuria occurred, nor could any deleterious effect upon the corpuscles be detected in specimens of blood taken at frequent intervals during and after anaesthesia.

In this connexion a rather interesting fact was observed. It was found that although a 10 per cent. ether solution sometimes produced haemoglobinuria if this strength was used throughout the operation, if the infusion was begun with a weaker solution, say 5 per cent., then the strength rapidly increased to 10 per cent., no such result occurred. This phenomenon rather suggests that the haemolysis produced by the strong solution takes place just at the beginning of the operation in the vein, close to the cannula, where the concentration of the ether is comparatively high, before it becomes diluted by mixing with a large volume of blood. If this were not so, it is difficult to see why the haemoglobinuria is slight; one would suppose that if a solution mixed with blood were capable of destroying a few corpuscles it would destroy them all.

A continuous flow of solution was originally obtained by the use of two reservoirs, one containing ether solution and the other saline, linked together so that when the ether was turned off the saline could be turned on. This, of course, maintained the continuity of the stream and got over the difficulty of the blood clotting in the cannula, but all the objections coincident upon the use of an interrupted dose of anaesthetic were still present. An explanation of the method by which the stream of ether saline is graduated and delivered continuously involves a description of the apparatus employed.

This consists of a reservoir, suspended about 8 ft. from the ground, to contain a supply of the ether saline solution. This reservoir is connected by a rubber tube to a regulating chamber which also acts as an indicator of the amount and rate of flow. This regulator is constructed upon the well-known principle of a motor lubricator. The fluid runs through a pipette passed through a cork in the upper extremity of a glass bulb. The stream then falls through the centre of this bulb, and finally leaves it by an aperture at the lower end. Just below the bulb a tap is placed by means of which the rate of flow is regulated. Although if the fluid is only drifting slowly through the regulator the continuity of the stream is interrupted, the pressure from the upper reservoir is nevertheless transmitted by the cushion of air contained in the regulator. Leaving the regulator, the fluid passes on to a warming chamber, and then on to the cannula which is introduced into a vein.

The apparatus is perhaps a little complicated, but this is, I think, not altogether a disadvantage. It can easily be taken to pieces and sterilized by boiling. In a method such as this, which entirely depends for its safety and efficiency in attention to details and the most rigid asepsis, simplification at the expense of accuracy might easily lead to its adoption under more or less adverse circumstances with considerably greater peril to the patient than would be the case if some more orthodox method of anaesthesia were employed. For it is obvious that to attempt a surgical proceeding of this sort, involving as it does opening of the cardio-vascular system, without sufficient assistance and every facility for asepsis, is to court disaster.

Induction of Anaesthesia.

The general preparation of the patient is the same as for inhalation anaesthesia. With regard to the special preparation, the question arises as to the advisability or otherwise of giving certain alkaloidal bodies previous to operation, with the view of insuring the greater comfort of the patient before and after anaesthesia and a better general condition during operation. If good results are to be obtained with this method of giving ether, it is always necessary to give a full dose of atropine sulphate previously to stop the secretion of mucus, otherwise salivation frequently occurs with all its ensuing train of difficulties, if not dangers.

The judicious combination of morphine and scopolamine with the preliminary atropine injection undoubtedly improves the anaesthesia and after-effect. But the discussion of the point brings out very clearly how difficult it is to attempt to establish a routine combination of anaesthetics or narcotics.

These last mentioned drugs are powerful poisons and require to be given with the same degree of foresight before a surgical operation as they do during the course of a medical case. In certain selected cases the most excellent results are obtained by the use of morphine and scopolamine, separately or together, while in other unsuitable ones they may be responsible for the very gravest danger. The best results are undoubtedly obtained with this method of giving ether if the patients are previously prepared by a carefully selected combination of alkaloids.

After the preparation of the patient the next question that arises is the actual induction of anaesthesia. In order to effect this the cannula of the apparatus must be introduced into a suitable vein. For this purpose either a glass cannula or a sharp, hollow steel or nickel needle may be employed. A certain difference of opinion exists as to whether it is better to plunge a sharp metal needle directly into the vein, or to cut down and isolate the vein and tie in a glass cannula. Here again I think it is a question of individual cases—if the vein can be well seen and the operation is likely to be short, with little movement of the patient, then it is quite easy and very convenient to use a sharp needle directly into the vein; but if, on the other hand, the vein is covered by subcutaneous fat, or the patient is very emaciated or the position is likely to be altered during operation, then it is certainly preferable to expose the vein under a little local anaesthetic and tie in a glass cannula, as nothing is so trying as leakage into the subcutaneous tissue during an operation, and this state of affairs necessitates cutting down and changing from the metal to the glass cannula during the operation under much greater difficulty than is the case if this is done calmly before the operation starts. So I think it fair to say that the glass cannula tied into the vein is more certain, although in selected cases the needle may be more spectacular. A cannula having been introduced by one means or another into a vein, preferably the median basilic or cephalic, the solution is allowed to flow in steadily; in other words, the tap of the apparatus is turned full on, until anaesthesia is obtained in two and a half to three minutes. The ordinary signs of surgical anaesthesia occur; when this state has been established a little experiment for each case will show the minimum amount required to maintain this condition and the apparatus can be set to deliver this amount.

There is little to be said as regards management of the case; it is similar to an inhalation anaesthesia, more or less being given according to the needs of the patient as they arise. Of course, a free airway must be maintained with the same care as with other forms of general anaesthesia. It may, perhaps, be pointed out in this connexion that although undoubtedy a safe method, it is far from being an automatic one.

The time which elapses before consciousness is regained varies with the composition of the preliminary hypodermic injection: if neither morphine nor hyoscine has been given recovery is extremely rapid, patients coming round

almost as quickly as from nitrous oxide gas; but if for any reason a long sleep after the operation is required, it can be procured by the administration of the aforesaid drugs.

Surgical anaesthesia can usually be obtained in about two and a half to three minutes; of course, this is only an average time—children and weakly individuals go under quicker, while others of a more robust type naturally take longer. The induction is usually quiet, especially so if hypnotics have been given beforehand. If excitement does take place, it is more easily controlled than with inhalation methods, because the continued introduction of the ether is not hindered by the respiratory spasm which accompanies the struggling, and the well-known dangers of a rapid induction by chloroform do not apply in the case of ether. The extreme flexibility of the anaesthesia produced by infusion is one of its chief recommendations. The patient can be transferred from deep to light anaesthesia in a period of time that can almost be counted in seconds. This, to my mind, is the chief objection to some of the other compounds used for infusion; the anaesthesia is not flexible, and it is necessary to work throughout the operation at the greatest depth of anaesthesia required.

With regard to the safety of ether infusion, we know statistically that ether is a safe drug; we also know that saline infusion, aseptically carried out, is not attended with danger. So that really we are not dealing with quite a novel method, and consequently we have other grounds besides the actual number of cases already collected for assuming the safety of the proceeding. Personally, I have never had any cause for anxiety when using this method, and have never seen any accident other than an overdose, which was immediately recovered from on turning off the ether. I think that the degree of muscular relaxation given by the infusion anaesthesia is precisely the same as that obtained with ether by any other method, and depends upon the depth of anaesthesia and the amount given. It is impossible to say the relaxation is equal to that given by spinal anaesthesia when stovaine is employed for the purpose, but it is certainly equal to that given by other inhalation anaesthetics.

The following is a summary of cases up to the present date carried out upon the principle which has just been described. The great majority of the cases have been collected from the records of University College Hospital; and I am indebted to the kindness of my colleagues for permission to uses their cases as well as my own for the purposes of this paper:

No. of Cases.	Operation.	Induction.		After-effects: Vomiting.
		Time.	Excitement.	
		Minutes.		
198	Abdominal	2-4	2	3
76	Head and neck	2-4	3	2
4	Thorax	2-4	—	0
19	Other operations	2-4	—	0
297				5

Amount of Anaesthetic.—About 1 pint per hour; in the shorter cases relatively more, as more of the anaesthetic solution is always used in the first than succeeding hours.

Course of Anaesthesia.—Smooth and regular; no tendency to cyanosis.

Period of Unconsciousness after Operation.—Varied from a few minutes to three hours, depending upon whether narcotics had been previously given.

It is sometimes a little difficult to see the relative uses and disadvantages of a new medical or surgical method in their true perspective; and it seems that one of the most important questions connected with the subject is a careful consideration of the indications for the employment of this method of anaesthesia. Perhaps it would be well to enumerate shortly the advantages of this method:

1. Absences of deleterious after-effects both gastric and pulmonary.
2. Introduction of a quantity of saline fluid, by means of which shock may be either combated or prevented.
3. Absence of pulmonary irritation.
4. In certain cases the separation of the spheres of activity of the surgeon and anaesthetist.

Infusion anaesthesia is undoubtedly more complicated than other methods, and it hardly seems expedient to urge its use as a general rule unless there are definite indications. In hospital, with every facility at hand, it is, perhaps, a different matter; but for an ordinary case, say a hernia in a healthy adult, it would hardly be indicated in the absence of special circumstances, such as a previous anaesthetic with unusually severe after-effects.

The probable length of the operation is another factor to be considered; for infusion anaesthesia the ideal length is one and a half hours or under, for during the longer operation, lasting two or three hours, especially if upon larger and muscular individuals, several pints of solution may be required.

Apart from some of the general benefits mentioned above, which may be regarded in the light of a luxury and not a necessity, it is quite certain that the best results are obtained in those cases in which the patients are very bad previously to the operation—for instance, in cases of ruptured gastric or duodenal ulcer, acute appendix, intraperitoneal haemorrhage, or severe accidents these patients frequently leave the table in a much better condition than they came to it, and I think that it is safe to say that several of the patients recorded above owed their lives to the infusion.

In long operations involving much shock, I think the problem must be decided upon the merits of each individual case, and rather turns upon the question of how much fluid it will be necessary to inject, so that while the infusion might be adopted with great benefit for a combined abdominal perineal operation in a frail woman, equal advantages might not be gained for the same operation upon a plethoric and muscular man. This method is a very convenient one for certain operations about the head and neck, especially for those patients who are already in a feeble and emaciated condition. But, undoubtedly, the infusion brings about a greater tendency to oozing of blood; consequently, there are certain objections to its use for such cases as removal of the Gasserian ganglion, etc., where a little extra haemorrhage may materially affect the successful issue of the operation.

Although this method of anaesthesia has not been long in general use, it has been subjected to a fairly extensive trial, and I think I may justly say that all those who have had any extensive experience of the method speak of it with the warmest praise, and it seems probable that it will take its place as one of the recognized methods of inducing surgical anaesthesia.

DISCUSSION.

Dr. W. J. McCARDIE (General Hospital, Birmingham) said: With regard to the difficulties which arise from obstruction in or about the upper air passages during operations on the mouth, and for which advantage is claimed for the intravenous method, all of them, except those which affect the epiglottis and glottis, can be prevented by the use of Crile's tubes. In these operations on or about the mouth I find that difficulties in the glottic region rarely arise; but in operations on the larynx, or possibly large goitres, I think that intravenous anaesthesia offers considerable advantage. I agree that the method may be best for cases of shock or haemorrhage, in which the injection of saline fluid greatly helps.

Dr. VICTOR FIELDEN (Royal Victoria Hospital, Belfast) regretted that Dr. Rood had not shown the apparatus he used, for he had found that on introducing warm ether-saline solution into the chamber so much ether evaporated that the injection consisted of little more than saline solution. The proper heating of the ether solution was important. Had Dr. Rood seen any cases of oedema of the lung? The percentage of lung complications after chloroform quoted by Dr. Rood from Continental sources seemed to be very high.

Mr. ALEXANDER WILSON, F.R.C.S. (Manchester), pointed out the disadvantages that might occur in the way of respiratory embarrassment through the surgeon having uninterrupted access to the upper respiratory passages.

Dr. G. A. H. BARTON (Hampstead General Hospital) said: From the first I was much impressed by the potentialities of the intravenous infusion method of administering

anaesthetics. It gave me much pleasure, therefore, to listen to Mr. Rood's paper, read at the Royal Society of Medicine in the early part of the year, and I resolved to give the method a trial in my practice, and I had hoped by now to have had a sufficiently long list of successful cases to justify me in joining in a chorus of approval of the method. I regret to say the success has not been altogether unqualified, and the list is consequently only a short one. It may, perhaps, be none the less interesting and instructive on that account.

A few words first as to the apparatus and method adopted. Whilst admiring the perfection of Mr. Rood's apparatus, I found it too expensive and cumbersome for my purpose. Looking for something lighter, more generally adaptable, and less expensive, I found it in Mr. Page's hedonal apparatus, to which I added a warming chamber for use with ether. In practice I have found one disadvantage in Mr. Page's apparatus: though designed for boiling, its construction does not conveniently allow this method of sterilization. The glass parts are very apt to break; the original washers are made of a material which is disintegrated in the process; finally, a certain amount of fur is apt to be deposited on the interior of the reservoir. Having regard to the risks and delay involved, I have for the most part sterilized the apparatus by filling the reservoir with 1 in 20 carbolic and slowly running it through, afterwards washing out with sterile water, and boiling the cannula and last length of tubing only.

There is one minor inconvenience about the intravenous method which militates against its general use in hospital, where there is usually a long list of operations to be got through—that is the little preliminary operation necessary before the infusion can start. I have generally considered myself lucky if I have the patient ready for the surgeon within ten minutes of his being put on the table. To obviate some of this delay I have on a few occasions attempted to canalize the vein with a sharp needle without dissection. When the veins are prominent this may be done, but even then I have found that later on there is a chance of the sharp point fouling the walls of the vein and causing further delay or abandonment of the method. To prevent this I have had made for me a needle of larger bore than Bailie's and having an inner blunt cannula, which can be thrust beyond the point when the needle is *in situ*. This answers very well in many cases, but there will always remain a good proportion in which, owing to the veins not standing out well, it will be best to dissect them out.

To the intravenous infusion of ether there is, to my mind, another rather serious drawback—that is, the impossibility of infusing at blood heat. Wherever I have tried to do this the ether has bubbled out of the solution at an uncontrollable rate. In my first case the infusion had to be stopped half an hour before the completion of the operation, owing to the amount of ether vapour that suddenly got into the last section of tubing. Very fortunately, thanks to the preliminary injection of scopomorphine, the operation was easily concluded without any further anaesthetic being required. I cannot help but think that the infusion directly into the blood stream of large quantities of fluid below body temperature must have a depressing effect, especially in long operations involving some shock. However, I am ready to admit that this has not been particularly brought out by my own cases. I have only noticed two points bearing on this question. Of my 13 cases, 3 complained of pain in the arm directly the infusion started; whether this was due to its relative coldness I cannot say, but I suspect it. The other point is that during the recovery from the anaesthetic three patients either complained of feeling cold or looked and felt cold and pinched. In hardly any case did the patient appear stimulated during the administration as one would expect from a combination of ether anaesthesia with saline infusion.

A word as to the strength of the solution. I started by using 7½ per cent. solution in the first 4 cases. Afterwards I used a 6½ per cent. except in 1 case, a fragile woman, where I used a 5 per cent., and a very big, stout man, where I reverted to the 7½ per cent. In all the cases the induction was satisfactory, and there was no struggling of any consequence. The time was generally from three to seven minutes, and the amount varied from 200 c.cm. to 600 c.cm. As regards the anaesthesia, it was absolutely satisfactory in 8 cases. In 5 there was some clenching of jaw and retraction of tongue or laryngeal spasm, causing cyanosis; in 1 case oxygen had to be administered. In one or two cases there was some rigidity or slight movements, and in one a tendency to retch during the operation, but this was for a parotid tumour pressing on the pharynx, and the nature of the operation no doubt accounted for the retching. Take the cases all round, I may say that the anaesthesia gave the surgeons complete satisfaction.

I will now deal with recovery and after-effects. Here, I regret to say, the picture is not so pleasing. As one of the chief advantages of intravenous infusion was held to be the abolition of after-effects, my experience has, I regret to say, caused me some disappointment.

In 3 cases the recovery was excellent, and there was no vomiting or after-effects of any kind. In 3 there was a little faintness, coldness, or trifling vomiting during the recovery. In 6 cases vomiting gave a good deal of trouble, and the curious part was that in the majority of these patients the recovery appeared to be excellent, and when I saw them from one to three hours after the operation they made no complaint, and yet afterwards had more or less severe vomiting lasting for several hours, and accompanied in some cases by headache. One or two patients have complained of pain at the site of infusion, but I am glad to say there has been no real trouble with any of the arms.

In 3 cases there were serious after-effects, but how far due to the method it is difficult to say. One was a thyroidectomy for Graves's disease. She was very ill and extremely excitable. Forty-eight hours after she developed symptoms pointing to a low type of pneumonia, though no physical signs were discovered in her chest; her condition precluded a thorough examination. Ultimately she recovered after being in very grave danger. The condition came on very suddenly and the possibility of pulmonary embolism was considered. Mr. Colledge, under whose care the patient was, kindly gave me a full account of her progress, and concluded by saying, "I think the anaesthetic should be acquitted." In one patient haematuria supervened the day following operation. It was very free, clots forming in the bladder, and lasted forty-eight hours, but the patient seemed none the worse for it.

My last case brought the method into some little disrepute with the surgeon, Mr. Jackson Clarke, and caused us to hold our hand. No doubt the anaesthetic was to blame, but I doubt whether the particular method of administration had anything to do with it. However, it was the second case in a very short series in which there was haematuria—real haematuria, not haemoglobinuria. The operation was plating an ununited fracture of femur. The patient was a big, fat man of 61, pale and flabby. He had been operated upon three months previously, when I gave him scopomorphine and C.E., and he did very well. His urine had been examined, and he was not suspected of any renal disease, though very possibly his kidneys may have been somewhat cirrhosed. He had a preliminary injection of scopomorphine and was infused for two hours with a little over 2 litres of a 7½ per cent. solution. The anaesthesia was excellent. The pulse was rather poor and his colour at first rather dusky and later pale. He got rather cold towards the end. He vomited for twenty hours after the operation and then got suppression of urine and haematuria. For eight days the daily quantity of urine varied from ℥j to ℥vi only, and then gradually increased. During five of these days he was sick from two to five times daily. No doubt he had an acute nephritis. Eventually he recovered sufficiently to leave hospital.

On the whole, I am convinced that the after-results of scopomorphine and open ether are decidedly superior to those of the method under discussion. I trust I shall not be going beyond the limits of the subject under discussion if I make a brief reference to my experience with hedonal. This has been limited to 10 cases. The inductions were for the most part quiet and satisfactory, in two or three instances I noted the slight transitory duskiness which has been observed by others, but the majority simply seemed to fall into a quiet natural sleep from which in many cases they were partially roused by the application of the surgeon's knife, as shown by slight reflex movements, generally quickly abolished by increasing the rate of flow.

In one case, although the patient seemed well under, her movements were continuous throughout the operation, were not abolished by increasing the flow, and required restraining. In spite of the apparent lightness of anaesthesia, this patient took twelve hours to recover consciousness, and was drowsy the whole of the next day. In another case, a big woman weighing over 16 st., 1,050 c.cm. were infused in nineteen minutes without producing complete anaesthesia. The operation was concluded under the influence of C.E. mixture, a very little being required. In two other cases I have had to conclude with C.E. mixture; in one the cannula came out of the vein whilst the

patient was being turned on to his face, and in the other the supply of hedonal solution ran short. The amount required was again quite trifling.

On the whole, with the exception of a few cases where there were slight early movements due to an insufficient amount of the drug having been administered, the anaesthesia was characterized by great quietude and muscular relaxation. The respirations were particularly quiet, jaw relaxed, and tongue inclined to fall back. Hedonal must, I think, cause some depression of the respiratory centre, as any obstruction to the airway excites no response, and the laryngeal reflex is lost early. As regards the corneal reflex, I generally aimed at keeping it present, but was not always entirely successful. Very little change was noted in the pulse. In one severe abdominal operation in a bad subject I was pleased to note entire absence of shock. The main feature about hedonal infusion is the slowness of recovery from its effects. Three of the cases took about twelve hours before recovering consciousness. This was looked upon as an advantage in certain orthopaedic cases involving, as a rule, much post-operative pain. On the whole, however, I regard it as a grave disadvantage, as in the nature of things the anaesthetist cannot remain on the spot all these hours. In only one case was there a little vomiting. In the cases which had to be supplemented with C.E. there was much restlessness.

In one case the temperature gradually rose during the first twenty-four hours to 102°, at which level it remained for twenty-four hours and then gradually fell; there was nothing to account for this from the surgical point of view. In one of the cases which took twelve hours to recover the pulse during this period was extraordinarily rapid and rather weak, causing some anxiety. She was a very delicate child, and the pulse had been quick before and during the operation.

The majority of the patients caused no anxiety during the recovery stage and woke up feeling quite comfortable, as though from a refreshing slumber.

The last case calls for special notice, although, in the limits of time available, it is impossible to go into it fully. Briefly, the facts are these:

The patient was an ill-nourished tailor who had lately been on strike. There was no complaint of cough, and his respirations and temperature were normal; nothing was noted amiss with his lungs. Pulse was, of course, rather poor. Operation was opening and draining both frontal sinuses, the posterior nares being first plugged with a sponge. The induction was very quiet; in three and a half minutes and after the infusion of 400 c.cm. he appeared to be under. Rate of flow was slowed down, and at the end of five minutes he had had nearly 500 c.cm., and operation was commenced. The rate was now cut down to slowest possible, and remained at that. It was apparently one of the quietest and best anaesthesias I had had. The only adverse point about it was that his colour was dusky throughout. I attributed this at the time to the post-nasal plug and a towel over the mouth causing some obstruction. The operation lasted one and a half hours; 1,000 c.cm. were infused in all. A quarter of an hour before the termination of the operation I ceased the infusion and administered oxygen, which speedily improved his colour. No blood got into his air passages during the operation; the post-nasal plug was removed at its termination. I left him still unconscious, but apparently in good condition. He died two hours after without regaining consciousness. Without entering into all the details, the principal facts elicited at the autopsy were that the lungs were absolutely studded with small miliary tubercles and his air passages contained a quantity of fluid blood. This was not churned up and frothy, as it would have been if any effort had been made to overcome the obstruction, nor was the slightest attempt made to cough it up. I do not consider that death was due to an overdose of hedonal, but the depression of the respiratory centre and the delayed recovery (present in all these cases) were important factors in this unfortunate occurrence.

To sum up, this brief experience has led me to certain conclusions. The intravenous method is a good one, and I think may yet replace the inhalation method, but not with the drugs at present in use. Ether offers no special advantages either in regard to the type of anaesthesia or the after-effects. Hedonal is dangerous—quite as dangerous, I think, as chloroform, if not more so—owing to respiratory depression and delayed recovery. It should never be used in cutting operations on the upper air passages, nor, I think, in feeble subjects or those suffering from any disease of the respiratory system. When used the flow should be cut down to the slowest drops as soon as the patient is apparently asleep, and only increased when necessary to prevent reflex movements. The corneal reflex should never be abolished. In selected cases I think both will continue to be of value. But before the intravenous method supersedes the inspiratory one some anaesthetic will have to be found combining the safety of ether with the many advantages of hedonal.

The PRESIDENT said: My experience goes to show that the method of infusing ether and saline intravenously is a valuable one when employed with circumspection. Like all new methods, it requires a complete mastery of its technique, as well as a very careful selection of cases for its use. Undoubtedly, there are dangers peculiar to it, but these are capable of being avoided when the administrator is on the look-out for them and knows how to prevent them. It is essential that the anaesthetist should give the whole of his attention throughout the narcosis, even more so than in the case of other methods. The obvious advantage of infusing ether, rather than such drugs as hedonal, is that its effects are fugacious, while hedonal and its congeners induce a drugged condition, from which the patient cannot be aroused rapidly if circumstances arise rendering this desirable. With ether, also, it is easier to vary the depth of the narcosis according to the exigencies of the operation—for example, increasing shock. Mr. Rood has mentioned the serious post-operative effects sometimes ensuing upon chloroformization, and has given statistics collected from various sources. The value of such figures is, however, very slight, unless the statistics include in their purview the actual methods of administration employed and the actual quantity and strength of the vapour administered. Many, if not most, of the cases in which such complications ensued were the result of chloroform toxaemia and not of chloroform anaesthesia. Such sequelae can be minimized or abrogated, if only such an amount is given as will ensure anaesthesia and fall short of toxaemia.

DISCUSSION ON THE EXAMINATION OF PATIENTS BY ANAESTHETISTS.

OPENING PAPER.

By ALEXANDER WILSON, F.R.C.S.,
Anaesthetist to Manchester Royal Infirmary.

I AM sure it will be readily admitted that the advances in modern surgery, especially the practice of rigid aseptic surgery, have made the task of the anaesthetist harder and increased his responsibilities.

Not only are his duties more arduous from the extensive nature of the operations performed, but his opportunities of observation and freedom of action while at work are curtailed.

Where formerly during an operation a full view of the patient and area of operation was possible, now he is fortunate if he has free access to the patient's head. Too often even this is securely concealed under a sterile towel, and the necessarily somewhat septic anaesthetist, sitting at arm's length, can assure himself of the continued existence of the patient by the flapping of the valve of some inhaler and the pulse felt at the facial or temporal artery.

Further, increasing knowledge tends to saddle the anaesthetic and its administrator with the responsibility for sundry serious after-effects the real production of which is still uncertain. In addition, new anaesthetic agents have been introduced and new methods evolved for the administration of old ones.

The whole subject of anaesthetics and their administration has become more complicated, the choice has become greater. The methods have increased in number and variety. In these circumstances it is only by seeing the patient before operation that the anaesthetist can do the best for him.

DISCUSSION.

Mr. L. KIRKBY THOMAS (Birmingham and Midland Hospital for Women) said: The opener of this discussion has dealt most succinctly with the various reasons for affording the anaesthetist an opportunity of seeing and examining the patient previous to operation. To me it is almost incredible that any one who has had much to do with the administration of anaesthetics can fail to appreciate the importance of the subject under discussion—so incredible that I ask the question, Why is

it that the anaesthetist does not, as a routine procedure, see the patient before he comes to the operating table? The reason for this seems to me to come under two headings—(1) the attitude of the medical public, (2) apathy on the part of the anaesthetist. The following incident illustrates the first of these conditions:

A few weeks ago I mentioned to a practitioner, whose patient I had been anaesthetizing, that this discussion was going to take place, and that I thoroughly believed in it. He became quite indignant, saying that he knew his patient, that the surgeon had seen him, and that they were quite competent to decide whether he was a fit subject for an anaesthetic or not. I attempted to smooth his ruffled feelings by explaining that I did not question his competency or that of the surgeon, but that there was something more in the administration of anaesthetics than the mere question as to the fitness or not of the patient to take an anaesthetic. He seemed rather struck with the idea, and I left him ruminating thereon.

This incident is not an isolated one; it represents the attitude taken by large numbers of general practitioners, and is largely due, in my opinion, to what I stated as my second heading—apathy on the part of the anaesthetist. As long as we are content to be called in to give anaesthetics in cases of which, as often as not, we know nothing till the operating table is reached, so long will the really scientific administration of anaesthetics be retarded, and there will be, even in the most skilled hands, a certain percentage not only of preventable fatalities, immediate or remote, but also of cases in which a better anaesthesia might have been obtained for the particular operation in hand. I do not for a moment ignore the fact that in many instances there are factors which preclude the possibility of our examining patients before operation—emergency in particular—but I also think there is a still greater number of cases in which a little tact and insistence on the part of the anaesthetists would enable us to examine our patient before operation, to the benefit of all concerned, and I welcome this discussion as a means by which we can put before the profession at large our reasons for, and ideas on, this important subject. The first advantage to be gained consists in making the acquaintance of the patient, apart altogether from what may be the best manner of administration. As is well known, many patients are in far greater dread of the anaesthetic than the operation itself, and I am sure that a quiet talk with the man whom they know is going to anaesthetize them brings them to the operating table with a confidence which would not otherwise be the case—a state of things very much to be desired. The next thing that appeals to me concerns the preliminary injection of drugs. I have no doubt, in my own mind, as to the benefit derived from an injection of scopomorphine before operation. The patient comes to the table in a state of comparative calm, less anaesthetic is required, a more even anaesthesia is obtained, and after-vomiting is undoubtedly less frequent. This, at any rate, is the general opinion I have formed as the result of my own observation. I think, however, that the dose of scopomorphine to be given is also important—a routine quantity is impossible—in every case the amount to be given should be carefully considered, not only in relation to the patient's idiosyncrasy, but also with regard to the operation in hand, and here I think the anaesthetist's experience should often be of service both to the practitioner in charge of the case and to the surgeon who is about to operate. The maximum dose I use is $\frac{1}{2}$ grain of morphine and $\frac{1}{100}$ grain of scopolamine, and, speaking generally, children should have one-twelfth of this, old people, debilitated and anaemic women one-half. As regards special conditions, quite small doses should be given in cases with organic heart disease, renal mischief, and for thyroid operations. Cases in which the Trendelenburg position is likely to be assumed for any length of time should also be given a small dose—one-third to one-half of the maximum—as the tendency to lividity and respiratory trouble is increased by the drugs, and I have had several cases of this kind which have caused me considerable anxiety. In my experience, the best time for this preliminary injection is an hour before operation, and I have yet to see the case in which, inhalation anaesthesia having been decided on, a preliminary dose of scopomorphine is contraindicated. I have advised the intrathecal anaesthesia chiefly in cases of chronic bronchitis and alcoholics for operations below the level of the ribs, including ovariotomies, hysterectomies, hernial repair, resection of intestine etc. So far I have no failures to relate as regards the efficiency of the anaesthesia produced, and the after-effects have been practically *nil*, being, in fact, limited to a little sickness a few hours after operation in 5 per cent. cases. I have always used tropacocaine 5 per cent. in normal saline solution, and have found this answered admirably. The anaesthesia lasts from one to two hours, and it has not been necessary to resort to a second injection or to use any other anaesthetic.

Mr. C. CARTER BRAINE (Charing Cross Hospital) thoroughly agreed with all the points mentioned by Mr. Kirkby Thomas. During almost a quarter of a century's work as anaesthetist he had only been called in two or three instances to examine his patient before anaesthetizing him. On several occasions other individuals had been called in to advise upon the anaesthetic to be administered, but the anaesthetist himself who had all the responsibility had been neglected. Again, it was frequently the practice of the operator or the general practitioner to administer drugs—morphine, scopolamine, strophanthus, etc.—before the administration, and not even to acquaint the administrator of the fact, and it was only when the anaesthetist found the administration departing from the normal and his patient getting into an alarming condition that he was acquainted with the fact that his patient had been drugged previously. The amount of morphine administered previous to the anaesthetic was frequently excessive.

Mr. C. A. LEEDHAM-GREEN (Birmingham) said: At the present time the public is only just getting educated to the importance of having the services of a skilled anaesthetist, and I am inclined to think it will take some further time before it regards it as necessary that a preliminary consultation, with its contingent fee, should take place. It has been suggested that a surgeon is not justified in administering drugs to his patient previous to operation, except after consultation with the anaesthetist. With this proposition I am afraid I am unable to agree.

Mr. W. DAKIN MART (Sheffield Royal Infirmary) said: I quite agree with what has been said about the previous examination of the patient, but I consider that one of the most important matters is to gain his confidence, and whenever possible I make a point of having a quite friendly talk with him, and so gain his confidence and take away his nervousness. In this manner I am sure the administration of the anaesthetic works with much greater smoothness.

Dr. W. J. MCCARDIE (General Hospital, Birmingham) said: Crile, who has done so much to broaden our views on anaesthesia, says that the stored-up energy of the body may be discharged by physical injury, emotional excitation, or physical exertion. Now we must very seriously consider this state of emotional excitation. The surgeon and anaesthetist do not attach anything like enough importance to it, and should study to treat and reassure the patient psychically for days beforehand, and to remove the emotional strain immediately before operation by the administration of sedative drugs. Crile believes that what he calls noci-associations can and should be removed, and the patient put in a state of anoci-association. He says that the management of a patient up to the point of anaesthesia should be only by those surgeons and nurses who are humanitarian psychologists, and that the anaesthetist should be even more of a psychologist, and preferably a woman, because of her sympathetic consideration. Now these are weighty words and worthy of our gravest attention, and with them I firmly agree. Crile says further: As to the choice of an anaesthetic, anoci-association may be further helped by administering the pleasant nitrous oxide instead of the suffocating ether, and telling him that, therefore, there will be little or no after-effect.

Dr. DUDLEY BUXTON (President) said he was fully convinced that the advantages following the admission of the anaesthetist into the counsels of those who were preparing a patient for an operation greatly outweighed the hypothetical objections raised by some of the speakers. In the "dark ages," when any one, a young house-surgeon, a nurse, or another, was jobbed in to give the anaesthetic,

(save the mark!) the anaesthetist was naturally a negligible quantity, and the attitude of the surgeon was one which, intentionally or not, was intended to keep the anaesthetist in his place. This attitude remained among a few, but most of the surgeons of the present day were keenly alive to the advantages of working with men of their own status possessing special knowledge and expert experience in their own line. The more numerous the opportunities the anaesthetist enjoyed of studying the patient, the more valuable did his advice become. Mr. Wilson had shown very fully one aspect of this, and had dealt with the important matter of the selection of the appropriate drug, and the most suitable method of exhibiting it under diverse conditions of the patient and operation. No such accurate choice could be made unless the anaesthetist either had the facilities extended to him for examining the patient some days before the administration, or had enjoyed the advantage of a consultation with the surgeon and family doctor engaged in the case. Another and most important aspect of the matter was that such a system enabled the anaesthetist to win the confidence of the patient, and to make him or her realize that he (the anaesthetist) knew his business, and was devoting himself earnestly to achieve the best results in the interests of the patient. The dread of being confronted by a stranger, who would hold their lives in his hand, was a natural and very real one to most patients, and this must militate against the harmonious working out a satisfactory narcosis, of which fear or "funk" constituted no slight danger. This feeling of apprehension or panic would be absent; if the patient had acquired confidence in the anaesthetist, the latter would be regarded as a friend, not a foe. To the anaesthetist himself the advantage must be great in every way, since he would have had the opportunity of studying at first hand the environing circumstances of the problem—a satisfactory and safe narcosis—presented to him; also, if he could discount the patient's fears his task had been shorn of much of its difficulty. The difficulty which had been mentioned about fees was really more theoretical than real. When the public realized that they were receiving substantial benefit from the professional services of the anaesthetist they would be as ready to pay for them as they were in the case of his surgical colleague. Indeed, if no fee were forthcoming, the speaker was sure that the anaesthetist's pride in his work would make him ready to forego monetary reward provided there existed adequate reasons for such a concession. The highly-trained scientific man who had adopted anaesthetics as his speciality was at least as worthy of reward as any other specialist of similar attainments and hospital service. The public were seldom made aware that the anaesthetist's work was of a nature which needed both scientific training, research, and wide experience, but when this was pointed out they were reassured and were prepared to pay for the benefit which they recognized was a real one. The result of this work made the anaesthetist of greater value to the surgeon, since it compelled the anaesthetist to recognize that given the patient's safety, his duty was to facilitate the surgeon's operations, since anaesthetics were essentially ancillary to surgery and their use or abuse could help to make or mar an operation. The speaker was sure that when the system suggested was carried out as a routine measure, the trinity of interests—that of the patient, that of the surgeon, and that of the scientific anaesthetist—would individually and collectively be promoted.

A COMPARISON OF METHODS EMPLOYED FOR INDUCING ANAESTHESIA AND ANALGESIA RESPECTIVELY.

OPENING PAPERS.

I.—C. A. LEEDHAM-GREEN, F.R.C.S.,
Surgeon to Queen's Hospital, Birmingham.

WE have reached a period when the interest in the further development of anaesthesia is again keen, for in recent times a number of entirely new and interesting methods have been advocated, the value of which, however, has not yet been fully established. Little wonder, therefore, that at this time the opinions of anaesthetists vary widely, and, indeed, are often conflicting.

Although I appreciated the compliment of being asked to open this morning's discussion, I at once saw that the task of producing a passable criticism of the comparative value of the various forms of anaesthesia was no easy one. I did not, however, expect to find it quite so difficult as it has proved. The fact is that, although the subject we are to discuss to-day is full of practical interest for all of us, it is one in which it is exceedingly difficult to obtain reliable data on which to base a comparison.

For instance, let us attempt to answer the first item in our comparison—What is the mortality of the various forms of anaesthesia? A few years ago, when practically all operations were done under a general anaesthesia produced by the inhalation of chloroform, ether, or a mixture of these, it was not difficult to give a statistical comparison which would be accepted by most surgeons as fairly accurately representing the facts. To-day any such comparison is rendered much more difficult, for not only have entirely new forms of anaesthesia been introduced, but even the old ones have been so altered, both by the introduction of different modes of administration and the increasing use of a combination or sequence of anaesthetics, that the critic finds it exceedingly difficult to collect satisfactory statistics on which to base his report.

Yet such figures must be forthcoming before definite conclusions can be arrived at. I have endeavoured, both by an exhaustive study of the literature and a widespread personal inquiry, to collect such figures, but though a study of them has helped to strengthen or weaken previous impressions, they are for the most part far too limited to be worthy of serious consideration.

We cannot hope to prove or disprove the supposed danger of a method of anaesthesia by quoting the statistics from any single clinic.

We want figures on a large scale from several sources if we are to avoid serious errors.

Among the most reliable statistics, based upon large numbers of anaesthesias, are those published by Gurlt, who at the instigation of the German Surgical Society made a careful inquiry on this point, and for seven years (1891–1897) collected and analysed the figures from a number of the large Continental hospitals. The total number of anaesthesias reported upon was 330,000, and of them chloroform gave a mortality of 1 in 2,075 cases, and ether 1 in 5,112.

Comparing these figures with a large number of statistics which I have obtained from several hospitals in London and the provinces, I find that the death-rate for chloroform in England seems to be a little higher, and that for ether considerably lower, than that given by Gurlt. Be this as it may, there is no doubt that chloroform is six or seven times as dangerous as ether, and as at present administered in most of our large hospitals is attended by very serious risk to life—so serious that a very grave responsibility rests upon those who use it as a routine anaesthetic.

That this is so when chloroform is administered in the old-fashioned drop method has long been known, but does it hold good when the dosimetric method of administration is followed? Probably not, but that is a point upon which I shall be very glad to have more information. Up to the present I have been unable to find reliable statistical evidence that dosimetric administration, whether combined with oxygen or not, has been followed by a marked diminution in mortality.

This was also the conclusion which Neuber came to from a study of the results of 70,000 anaesthesias collected by him in 1909. The mortality came out practically the same as that given by Gurlt—namely, for chloroform, 1 in 2,060, and for ether, 1 in 5,930. That is to say, the widespread use of Roth-Dräger's dosimetric apparatus had not been followed by any appreciable lowering of the mortality.

Notwithstanding the large amount of literature which has appeared on the subject of spinal anaesthesia, we seem still far from knowing the true value of this method. At present, in spite of the numerous collective investigations, it is difficult to form even a rough idea of the mortality attending its use, the published statistics differ so

enormously, ranging from 1 in 200 (Hohmeier) and 1 in 17,847 (Thomaschewski). The true mortality probably corresponds to about the statistics given by Strauss—namely, 1 in 2,524, and Crelaujak 1 in 2,086. This being so, it would seem that the method has practically the same mortality as chloroform.

From very extensive reading on the subject, and as the result of personal inquiries, I think there can be no doubt that spinal anaesthesia is slowly declining in favour. And, although there are some surgeons of the highest repute who still regard it as the best form of anaesthesia for general use, and who are able to point to huge numbers of successful operations carried out under its influence, yet the wave of enthusiasm for the method seems to be receding.

A longer experience of it has shown that, whilst it has undeniable advantages in certain cases, its use as a routine anaesthetic is accompanied by serious disadvantages.

Apart from the question of risk to life, the method from time to time gives rise to a number of distressing after-effects, such as intense and long-continued headache, severe collapse, temporary paralysis of the ocular muscles, the bladder, rectum, etc. Albuminuria, which so frequently follows the administration of chloroform and ether, is also exceedingly common after spinal anaesthesia, especially after the use of stovaine, according to Schwertz in 78 per cent. of all cases. This albuminuria, which is accompanied by a slight necrosis and desquamation of the renal epithelium, is, however, of a transient nature and of little clinical significance. A certain number of nervous disturbances, such as neuralgia, headache, vertigo, and epilepsy have also been recorded as developing several months after spinal injection.

Severe after-effects are certainly rare, and many surgeons, like myself, can point to several years' experience of this method without ever meeting untoward results. But the fact that these severe and permanent after-effects do sometimes occur, and occur in the practice of those whose technique is irreproachable, must have consideration, and, as far as I am concerned, it has caused me to give up the method, save for exceptional cases—an attitude which finds an increasing number of supporters.

The method is indicated in major operations upon the legs and lower part of the trunk in aged and feeble patients who suffer from chronic alcoholism, nephritis, diabetes, and chronic respiratory trouble. For such patients I prefer it to all other forms of anaesthesia.

The unconsciousness of anaesthesia is often likened to that of natural sleep. Unfortunately the resemblance is but superficial. The sleep of Nature is a period in which reparative processes go on in the cells of the body; in that of anaesthesia, on the contrary, much destruction takes place. All the anaesthetics we employ are protoplasmic poisons injurious to cell life. The toxic effect varies not only according to the dose and concentration of the drug, but also according to the vitality of the cells.

By reducing the amount of anaesthetic given to the minimum necessary to obtain the desired effect, and by shortening the duration of the narcosis as far as possible, we are able to diminish its deleterious effects to a marked degree, but we cannot avoid inflicting a certain appreciable injury on the organism as a whole. Still greater is this injurious effect when the resistant power of the cells has been already reduced by the action of septic toxins or other causes.

It is true that, as a general rule, the constitutional disturbance caused by an anaesthetic is but slight and readily escapes the notice of the unobservant, and, provided the patient is reasonably healthy, no serious effects are likely to follow. But, on the other hand, again and again the recovery of a patient seems to hang upon a hair, and the slightest adverse influence is sufficient to turn the scale. One sees this especially in the aged, the cachectic, and the anaemic, where the recuperative powers are notoriously low, as well as in the alcoholic, the nephritic, and the obese. In these and many other types the success or failure of the operation may depend on the nature of the anaesthetic and its mode of administration.

Of the general anaesthetics, there is no doubt that chloroform is decidedly the most toxic and the most frequently followed by serious after-effects as a result of this cellular degeneration. Its injurious effect upon the myocardium and the muscular tissue generally, upon the stomach and mucous membrane, the liver and other viscera, is too well known to need further reference here. There is, however, an impression in England that chloroform affects the kidneys less severely than ether does, and that it is therefore a preferable anaesthetic for patients suffering from renal inadequacy. This is a mistake; both of these anaesthetics affect the renal tissues adversely, producing degeneration and necrosis of the epithelium. But ether does so to a less degree than chloroform, and is therefore to be preferred when the choice is limited to these two drugs. The only organs which seem to be more seriously affected by ether than chloroform are those of the respiratory system. The vapour of ether has a decidedly irritating action on the mucous membrane lining the air passages, and formerly, when it was customary to give this anaesthetic as a highly concentrated vapour by means of a closed apparatus, respiratory troubles (bronchitis and bronchopneumonia) were frequent, and often led to a fatal issue. Now that it is usual to give it freely mixed with air or oxygen, and also to check the salivary and mucous secretions by a preliminary dose of scopolamine or atropine, the irritating action of the ether is greatly reduced, and respiratory troubles following its use are but rarely seen.

In the hope of still further reducing this possible trouble it has been recently advocated that the ether be given either by the rectum (an old method mentioned by Pirogoff in 1847 and long since discarded) or intravenously, by means of 5 per cent. solution of ether in saline fluid. Personally, I think it exceedingly doubtful whether either of these methods is of real practical value, the chief advantage claimed for each being the avoidance of the irritating action of the anaesthetic upon the respiratory tract of patients who are specially sensitive. Unfortunately, however, the greater part of the ether, however administered, is eliminated through the lungs, so that it is doubtful whether the small advantage gained in this way compensates for the many obvious objections to both of these methods.

It would, of course, be absurd to ascribe all the parenchymatous degeneration and metabolic disturbances following an operation solely to the anaesthetic. Many other factors contribute to these results, such as the presence of septic and other poisons in the blood. But the unfavourable influence of these toxins is undoubtedly exaggerated by the action of the anaesthetic, which lowers the power of resistance generally.

The last few years have witnessed a revival of interest in local anaesthesia, thanks to the work of Reclus, Schleich, Braun, and others. The technique has been so improved that a large proportion of even major surgical operations can be performed under its influence, not only without pain to the patient but often with greater ease to the surgeon than is possible under general anaesthesia. As it is almost the only form of anaesthesia which can be said to be devoid of danger, it seems somewhat of a reproach that so little use is made of it in this country. I may say, as some evidence of its value and scope, that in the first half of last year I find that I performed 40 per cent. of all my operations at the Queen's Hospital under its influence. The method is, of course, only applicable to certain operations. It can rarely be employed for intra-abdominal ones, and is unsatisfactory where the tissues are extensively inflamed. Moreover, the majority of patients prefer to be unconscious at an operation, wishing neither to feel nor to know what is done. On the other hand, the advantage of using a method which produces no impairment to the vitality of the organism is so great, especially when dealing with severely debilitated patients, that I would strongly urge its more general adoption.

In short, it seems to me that our principal task now is, on the one hand, to enlarge as far as is prudent the field of those methods of anaesthesia which may reasonably be considered free from all danger, and, on the other hand, more sharply to define the indications which call for the use of those anaesthetics which, however valuable, are unavoidably associated with a certain risk to life.

II.—Albert Ehrenfried, M.D.,
Surgeon, Consumptives Hospital, Boston, U.S.A.

THE INTRATRACHEAL INSUFFLATION OF ETHER.

It is now about three years since Meltzer and Auer first introduced the method of etherization by intratracheal insufflation, and sufficient time has elapsed for us to be able to give the procedure a definite place in surgery. I wish to speak merely of my own experience with the method. In the fall of 1909 I called on Carrel at the Rockefeller Institute with regard to some vascular experimental work which Dr. W. M. Boothby and I had undertaken, and he showed me his apparatus. This laboratory apparatus, modified so as to be efficiently controlled by the foot of the assistant, we adopted for routine use on animals, on account of the ease with which it is managed, the fact that no etherizer is needed, and chiefly because since we have used it in place of cone ether we have had practically no ether mortality.

After considerable experience with its use on animals, I became interested in the application of this principle of anaesthesia to human subjects, particularly in reference to my surgical work at the Boston Consumptives Hospital. Up to this time a few successful but desultory cases had been reported by Charles A. Elsberg, a New York surgeon who had been interested in the method by its discoverers, and who had evolved a rather complicated apparatus for its conduct. I felt that if the principle were to receive widespread acceptance it would have to be rendered as simple as possible, and, with simplification as the basic idea, I designed and built the apparatus which I showed at a clinical meeting of the Boston City Hospital, March 16th, 1911, and described, with a report of several successful cases, in the *Boston Medical and Surgical Journal* for April 13th, and later at the annual meeting of the Massachusetts Medical Society in June. With added experience various modifications have suggested themselves, and I present here to-day the perfected apparatus, as made for me by the Geo. P. Pilling and Sons Company of Philadelphia.

The apparatus consists of a Wolff bottle with three necks, sitting within a copper water-jacket, which is filled with hot water, and a foot bellows. By means of cocks on the outside of the jacket, the stream of air from the bellows can be carried either through the hot water, over the top of the ether (contained in the Wolff bottle), or can be made to bubble through the ether when a particularly strong vapour is desired. Air and ether vapour can be mixed in any proportion. Connected with the delivery end of the apparatus is a safety valve and pressure regulator consisting of a bottle of mercury, into which a tube is plunged. The depth of the tube in the mercury, which is adjustable, represents the maximum of pressure which is allowed with the apparatus; if, for any reason, such as spasm of the glottis, the pressure should rise, the valve "blows off" automatically, and danger from interstitial emphysema is avoided. In our early experience we employed a dial manometer registering in millimetres of mercury, to record the pressure, but the pressure bottle answers as well for all practical purposes. The apparatus is provided with a device to filter the air and to prevent droplets of condensed ether being carried over into the larynx. A clamp prevents the stoppers from becoming loose or blowing out of the bottle. The air or mixture is supplied at a practically constant temperature of about ten degrees above room temperature; if the operation is to last over three-quarters of an hour, the contents of the water-jacket may be readily replaced.

The simplicity of the thing is apparent. The original apparatus of Elsberg has certain drawbacks: it is cumbersome and expensive, depends upon an electric current for its motive force, and requires an electrical as well as an anaesthetic expert to run it. My apparatus, on the contrary, is portable, simple to operate, and of low cost, yet it is safe and efficient, and can be used anywhere that ether is given. In two of my cases the apparatus was run successfully by men who had never seen it before.

In addition it has some minor points of advantage over other machines—it can, on occasion, deliver a highly concentrated ether vapour; it is provided with a filter to prevent the possibility of any droplets of ether vapour which have condensed in passage through the tube being carried into the lungs; it is provided with a safety valve by which the intrathoracic pressure can be controlled, and which acts as a positive safeguard against any injurious effects of temporary or prolonged over-pressure on the lung tissue. This last appliance I consider of essential importance, particularly for men who are using the apparatus without much previous experience, as several fatalities from interstitial emphysema have come to my notice. Spasm of the glottis is apt to appear when patients are going under and whenever the concentration of the ether vapour is increased. At such times the safety valve blows off, and releases what might become a dangerous intrathoracic pressure.

For an intratracheal tube one can use either a soft rubber or French lisle catheter. I prefer the latter because it is smoother and its walls are thinner in proportion to its diameter. It should be rendered pliable by being soaked in hot water before using. It is pushed in gently until resistance is felt, and is then withdrawn about a half-inch. The proper size for an adult is 21 French. The tubes should be sterilized by boiling, as formaldehyde or chemical disinfectants may cause local irritation. Specially constructed tubes have been designed for the purpose, but I find that they possess no practical advantage over the catheter.

The chief drawback to the general application of this method of anaesthesia has been the difficulty in introducing the tube. After having tried various methods, all of which turned out to be unsatisfactory, I have followed a suggestion of Dr. F. J. Cotton, and made an introducer, a laryngeal forceps with sleeves attached for grasping the tube near its extremity, similar in principle to the introducer of Doyen. After considerable pains to produce the proper curve—working on frozen sections and cadavers—I have evolved an instrument which one can learn to guide into the larnyx in a matter of seconds, with the mouth gag in place and the left forefinger on the epiglottis, without the necessity of using a head mirror or electric illumination, or changing the position of the patient on the table.

For use with the soft rubber tube, I have adopted, with modifications, the introducer of Cotton and Boothby, which is a cannula made on the same curve as my forceps, through which the catheter is pushed. By means of either of these introducers any one should be able to acquire facility in passing the tube. This method has superseded all others in the hands of those using intratracheal anaesthesia in Boston.

When I first tried out this form of anaesthesia it had never before, to my knowledge, been used on human beings except by Dr. Elsberg. But my experience of its advantages in animal surgical work made me confident that it could be successfully adapted to human surgery, and I have never regretted having employed it. In a personal series of over 25 cases I have met no unpleasant experience. On a few occasions there has been difficulty in inserting the tube—to be accounted for by the fact that the patient was not sufficiently etherized—but with the tube once in place the rest has been easy. The patient sleeps calmly, with completely suspended respiration, or breathing a few times per minute, depending on the volume of air delivered at the bifurcation of the bronchi, but maintaining good colour and a good pulse. I have had patients under as long as one and a half hours. Breathing is resumed immediately the tube is withdrawn. No patient of mine has ever suffered any ill result from the use of this method. Recovery has been smooth, with little or no vomiting, and there has been no complaint of interference with the phonation from pressure of the tube against the vocal cords, or pulmonary complications of any sort.

The first indication for the use of this method is, of course, in thoracic surgery, and here it is bound to supersede the negative pressure cabinet of the pioneer, Sauerbruch, partly on account of its simplicity and adaptability, and partly on the wider margin of safety which it offers. For recent researches of Auer and Meltzer have shown that under the differential pressure method with the thorax open, air goes in and out of the thorax with respiration through the opening rather than through the trachea, which offers greater resistance, and the only portion of the lung which functionates is the lower lobe, which is in contact with the diaphragm. If this lobe is dislocated from its normal position, the patient is likely to die. With intratracheal insufflation the normal respiratory interchange is maintained whether or not the chest wall is intact.

When the thorax is opened the lung may be permitted

to collapse sufficiently to take it away from the chest wall and out of the field of manipulation (either by lowering the pressure at the bellows or by partly withdrawing the tube), and at the end of the operation the lung may be re-distended so as to drive out the air in the thoracic cavity and bring visceral and parietal pleura together, while the chest wall is being sewed up (by increasing the pressure, by inserting the tube farther into the trachea, or by pinching the trachea so as to obstruct the back flow). And with this method, as compared with the differential pressure cabinets, there are no bands of elastic constriction about neck or abdomen, and the anaesthetist has free access to the patient's face and mouth without interfering with the pressure. The operator works under normal conditions, and his field is not limited by apparatus; the anaesthetist may remain at some distance from the patient. If the heart stops beating from any cause, the most efficient form of artificial respiration apparatus is immediately at hand. The apparatus is not complicated by machinery which is likely to balk the operator, and with it there is no danger.

This form of anaesthesia has also a considerable scope of application outside the field for which it was originally designed. In operations upon the face, mouth, and neck, it has obvious advantages. The operating field is unobstructed; run at a low pressure, it will insure adequate ventilation of the lungs and obviate the possibility of aspiration of mucus or blood, and thus lessen the frequency of secondary pneumonia following face and mouth operations in the elderly.

My series includes amputation of the tongue for cancer, deep dissections of the neck, dissections of the tonsil, operations about the larynx, as well as the intrathoracic work for which it is primarily designed. The operator is at first somewhat disconcerted by the appearance of the patient, who lies upon the table motionless, with suspended respiration, and without making a sound, but this is overcome by the comfort of knowing that the patient is bound to get sufficient air under all circumstances, that a minimum of ether is being used, that there will be little or no after-sickness, and that no foreign matter can be aspirated into the lungs. The air passages are kept dry of mucus, and inhalation of mucus, blood, or vomitus is prevented by the return current of air about the tube. No more ether is used than is needed within the lung; the ether vapour is washed out with air as soon as the anaesthesia is over, and therefore recovery is rapid; the chance of excess ether or ether-laden mucus getting into the stomach is slight—accordingly recovery is gentle. The tongue and jaw do not have to be held, and there is no danger of the glottis falling back and closing the trachea. Added to this is the great convenience, in operations about the face, neck, and thorax, of not being hampered by the close presence and constant ministrations of the anaesthetist.

Since I operated for the first time in Boston under this form of anaesthesia, on February 23rd, 1911, the method has created a considerable interest and has been widely adopted, not only for thoracic work, but for head and neck work as well. The cases upon which it has been used in and about Boston number well over 100, and this series, with the 200 cases which Dr. Elsberg reported a year ago, establish the procedure on a firm basis.

The anaesthesia should be conducted or supervised by a responsible anaesthetist. There is no better training, of course, than to use the apparatus on animals, to acquire familiarity with it, before employing it on the human subject. For instance, a few thoracotomies on cats will give one excellent opportunity for direct observation of its effect on the lung. And the fewer the complications of the apparatus the easier will it be to learn its control.

In closing, I want to say only that this method of anaesthesia promises much. It not only opens up the field of thoracic surgery to the general surgeon, but it solves the anaesthetic problem with regards to extensive face, mouth, and neck dissections. The technique of its application should be mastered by all who may be called upon to do this class of surgery. Its general adoption will evidently be slow; meanwhile, it should be employed with discretion, but it is of undoubted value, and eventually its use will be widespread.

III.—ROBERT E. KELLY, M.D., B.Sc., F.R.C.S.,
Assistant Surgeon to the Royal Infirmary, Liverpool.

I HAVE used intratracheal insufflation for the past three months in various diseases, and on patients varying from the age of 10 to the age of 60.

My cases, 35 in number, include the following:

Operations on the head and neck: Partial excision of the lower jaw; excision of the upper jaw; hemi-excision of the tongue; removal of an epulis; thyroidectomies; removal of malignant glands in the neck.
Operations on the thorax: Excision of breast.
Operations on the abdomen: Gastro-enterostomies; cholecystostomies; ileo-sigmoidostomy; appendicectomies; herniae; excision of rectum; operation for fistula; kidney operations.
Operations on the extremities: Amputation through the thigh.

The above form a representative group of the cases occurring in a general surgical clinic. The practical procedure is as follows:

The patient is anaesthetized by the open ether method, having generally been given a preliminary injection of $\frac{1}{100}$ grain atropine. When the patient is well under, the tracheal catheter is passed.

It is sometimes difficult to see the glottis, especially in patients with prominent upper teeth, but so far I have not failed to pass the catheter down the trachea. At the moment of insertion there is a spasm of the glottis, with coughing, if the patient is not quite under. This spasm is momentary, and passes off as the stream of air enters the trachea. Should there be any mucus in the trachea the returning blast of air will immediately dislodge it. In patients with bronchitis, this clearing out of the trachea appears to be very beneficial.

For the first few moments, when there is spasm and coughing, I keep the pressure low and the percentage of ether high until the onset of regular breathing and the disappearance of the light reflex. I then raise the pressure to about 25–30 mm. Hg and drop the strength of ether to half or three-quarters. It is then possible to maintain the patient in a condition of anaesthesia somewhere between the loss to light and the sluggish corneal reflex.

The breathing is particularly light, and sometimes can only be appreciated by the rhythmic swish of the outgoing air current.

An attempt at a cough, a contracted pupil, a quick response to a touch of the cornea, or a prolonged inspiration mean, of course, too little ether, whilst a loss to light or touch indicates that the ether strength is too high. But the control over the latter is so exact that it is quite easy to maintain the constant depth of anaesthesia desired. I would like to refer to the feeling of absolute safety one has in working this machine, since on the slightest difficulty the putting back of the ether pointer to zero at once converts the instrument into a perfect artificial respiratory apparatus.

Meltzer originally stated that it was impossible to kill a dog by this method of anaesthesia, but he has since modified this statement. In order to kill the animal it is necessary to continue the insufflation for ten hours after all respiratory movements have ceased. It follows, therefore, that so long as even the slightest respiratory movements continue the patient is quite safe, and in any case there are no operations on the human subject which would last as long as ten hours.

When once the skin incision has been made, the amount of ether may be lessened until the operator reaches the parietal peritoneum or wishes to suture the skin at the end of the operation.

To demonstrate the impossibility of blood, mucus, or vomit getting past the glottis, I may mention the fact that by giving a dog an emulsion of charcoal and making it vomit under anaesthesia by an injection of apomorphine, not the slightest particle of charcoal made its way into the trachea, whereas with an ordinary anaesthesia, despite the greatest care taken to prevent it by posture or mopping, some charcoal always found its way into the air passages.

During the course of the anaesthesia the condition of the patient is all that could be desired. The colour is always a rosy pink, the pulse remains good, and no anxiety is felt about what is sometimes called by the Americans the "death space."

In order to give the heart as little to do as possible, the

pressure is dropped every few seconds to zero, so as to allow the great thoracic veins to fill. This turning on and off of this tap is all that one has to do. On no occasion has there been any vomiting during the insufflation. When the operation is over the ether is turned off and the lungs flushed with pure air. This is said to make the recovery time quicker. I have noticed this in some of my cases, but it does not seem to be constant.

After the tube is removed ordinary breathing ensues after a short interval.

As regards after-effects. Vomiting is extremely rare, and ether bronchitis did not occur in any of my 35 cases.

In an old man with enlarged prostate, with bleeding into the bladder, a suprapubic was done by the open ether method and the bladder merely drained, his condition being too bad for the radical operation. This small operation lasting only a few minutes, was followed by bronchitis lasting some days. Later, the prostate was removed under insufflation. No bronchitis followed.

A woman was admitted with acute intestinal obstruction, the result of strangulation in a large umbilical hernia. Her condition was so critical that a general anaesthetic was out of the question. Under local anaesthesia, therefore, I merely opened the first coil of dilated gut that presented. She gradually recovered from the intestinal obstruction. She was extremely fat, suffered from mitral disease and chronic bronchitis, and the umbilical hernia, which even now was as large as a football, was further complicated by a faecal fistula on its summit. Under insufflation the fistula was closed, a lateral anastomosis effected, and the edges of the ring overlapped—a prolonged operation lasting over one hour. Her chest condition was rather improved than otherwise after this long operation.

I feel satisfied that this method does no harm to the delicate mucous membrane of the trachea, and that it may be beneficial in patients whose breathing is difficult, or whose trachea is impeded by muco-pus.

Now as to the possible dangers: It is possible that if the catheter is rough, and during its introduction tears up the mucous membrane, there might be a danger of surgical emphysema.

With regard to ordinary emphysema, we must remember that the pressure as indicated by the manometer is not the pressure in the alveoli, for probably an increase of 10 mm. at the apparatus may only mean an increase of 1 mm. actually in the lungs, for the return is perfectly free. Moreover, a man blowing a cornet, or coughing strongly, may raise his thoracic pressure up to 160 mm. Hg, and this is far in excess of that ordinarily used, the average pressure being somewhere between 10 and 40 mm. Hg, and this measured by the apparatus, and therefore in excess of the pressure in the lungs.

The next danger is the introduction of the tube into the pharynx instead of into the trachea. It is for this reason that I practise the visual rather than the tactile method of introducing the catheter. One must see the glottis, and see that the catheter is correctly passed, otherwise there will always be the uncertainty that it has not been correctly introduced. By means of a Chevalier Jackson's direct laryngoscope this little operation is quite easy after a little practice. I always pass the catheter on a probe which almost fills its lumen, and I can then feel the grip of the glottis when I am withdrawing the probe. This, of course, would not occur if the instrument were in the oesophagus.

If the pressure of the air is too great, air may find its way into the stomach, but this may be obviated by the passage of a stomach tube, which may be left in during the whole of the insufflation, and does not interfere with the method. It has not, however, been necessary in any of my cases.

DISCUSSION.

Professor ARMSTRONG (Montreal) said: I have introduced the method of intratracheal insufflation into my hospital practice, and have used it altogether in some seventy operations with eminently satisfactory results to patient and operator alike. I find that operations on the lung are greatly facilitated. If it is required to pass the hand round any portion of lung between it and the chest wall, all that is necessary is to lower the pressure at which the stream of etherized air is delivered and the lung immediately recedes. This manipulation being completed, pressure is raised, and the lung distends till it comes in contact with the thoracic parietes again. Owing to the pressure at which the apparatus is worked, there is always a stream of air pouring out of the trachea between its walls and the catheter. The force of this effectually prevents aspiration of any foreign matters into the trachea; hence this method is of great value in operations in the oral or nasal cavities and upon patients who are the subjects of acute intestinal obstruction with constant vomiting. I also think the method has advantages in operations on large goitres, when the trachea is frequently compressed and respiratory troubles are apt to occur during enucleation. On the whole, I find that after-sickness is lessened by the insufflation method. I have never seen any ill results arise from the passage of the catheter within the trachea. In one case in which this method was adopted the patient was suffering at the time of operation from bronchitis and appeared to be benefited rather than otherwise. The noise made by the electric motor is certainly a source of annoyance whilst operating.

Mr. S. THOMPSON ROWLING (Anaesthetist, Leeds General Infirmary) said: The ideal state of anaesthesia is one of quiet breathing and muscular relaxation, with safety. There has been a long controversy on the respective merits of ether and chloroform as general anaesthetics to bring about this ideal state of anaesthesia. Without entering into this discussion, it will suffice to sum up the situation by the general conclusion that both anaesthetics have advantages and disadvantages, and that a combination of both has advantages over either separately (see Report on the Routine Use by the Open Method of a Mixture of Ether and Chloroform; Hewitt and Bloomfeld, *Reports on the State of Science*, 1909). The combination is physiologically sound, and clinically is found to stand the test of experience. The beginning of the administration with a mixture of ether and chloroform is less nauseous to the patient than with ether alone, and less dangerous than with chloroform alone; the stage succeeding loss of consciousness until deep narcosis is reached is the most dangerous of the administration, and should be met by giving the safer anaesthetic, ether; after deep narcosis is obtained a combination of ether and chloroform, or chloroform alone, is preferable to ether by itself. The great difficulty in using such a succession has been that the methods of administration of the two anaesthetics are different—for ether an inhaler, for chloroform a mask. The use of one or both in any proportion during the same administration is not practicable unless a uniform method of administration for both anaesthetics is used.

In the apparatus now shown this principle is fulfilled: both anaesthetics are obtained in the same way, each is vaporized by a stream of oxygen gas, and carried over to the patient in the form of vapour, and the stream of gas can be directed through either ether or chloroform or through both in any proportion by the turning of a tap. But it is found that an approximately definite percentage of vapour is given off in either case—of ether 40 per cent., of chloroform 20 per cent.; this percentage varies somewhat, chiefly with the temperature, but not sufficiently to materially affect the result. We thus get the basis for a calculation of the percentage administration in either case. These percentages—ether 40 per cent., chloroform 20 per cent.—are modified and reduced to the patient's requirements by the use of a special form of slotted angle-piece, which admits fresh air as a diluent. The angle-piece is attached to the face-piece, and has a rubber bag connected to the end distal to the slot, and into which the vaporized anaesthetic is received.

The further calculation is based on the fact that the normal respiration-rate is 10 litres per minute, and that the vaporized anaesthetic is admitted to the bag at a definite known rate per minute. As an example: When 1 litre per minute of ether vapour is admitted to the bag, and the air slot is so opened that the patient's breathing just fails to move the bag (that is, that there is no rebreathing), the patient will be breathing 1 litre of ether vapour *plus* 9 litres of fresh air. The percentage of ether vapour is thus reduced by nine-tenths, and the percentage of ether vapour breathed is one-tenth of 40 per cent. = 4 per cent.

By closing the air slot completely, the full 40 per cent. of ether vapour will be given and the bag will fill, but if the slot is left slightly open it will be found that the bag remains at the same fullness; it does not get more distended, and therefore as much is escaping as is being introduced; but one litre is being introduced, and this litre (40 per cent. of ether vapour) is now being diluted half and half—that is, to 20 per cent.; therefore, with the slot slightly open, the patient is breathing a 20 per cent. ether vapour.

With the slot more open, more fresh air is admitted, and the percentage of vapour falls from 20 to 16, 12, 8, and 4 per cent., this last being one-tenth of 40 per cent., and corresponding to the

position when the bag fails to move and the litre of ether vapour is being diluted with nine-tenths of fresh air.

With chloroform the percentage per litre is 20 per cent., but if only ½ litre is admitted this 20 per cent. is reduced to 5 per cent.; a reduction on precisely the same lines as for ether vapour is effected by the slotted angle-piece, the amounts being 2½, 2, 1½, 1, and ½ per cent., and corresponding to 20, 16, 12, 8, and 4 per cent. of ether.

It will be noted that these figures bear the proportion

$$\frac{8 \text{ ether}}{1 \text{ chloroform}},$$

and this is their approximate physiological strength. Thus, when the rate of flow through chloroform does not exceed ½ litre per minute, there is a maximum of 2.5 per cent. vapour, with the slot almost closed, and a working range of from ½ per cent. (slot open to no movement of bag) and 2 per cent. (slot four-tenths closed). When diluted with an equal rate of bubbling through ether, the rate may be increased to ½ litre, still giving the same percentage of chloroform, because diluted half and half. The percentage of ½ litre of ether is 5, 4, 3, 2, and 1 per cent., and corresponds to 2½, 2, 1½, 1, and ½ per cent. chloroform, according to the position of the air slot, and this is the rate used at first, to obtain unconsciousness. After this, ether vapour alone is used until deep narcosis is obtained, with the slot nearly closed, and in this way a liberal use of the powerful respiratory stimulant action of carbon dioxide may be made at the period when it is most needed—the dangerous period attending the early stage of deep anaesthesia. With oxygen this is possible without cyanosis, and the combination of ether, oxygen, and carbon dioxide is the safest that can be used. Subsequently, to *maintain* narcosis, a combination of ether and chloroform, or chloroform alone, may be used with safety.

In Appendix 3, Report of Committee on Anaesthetics, 1910, by Hewitt and Waller, the conclusions are: "We conclude with certainty that the intensity of the chloroform effect is greater than normal at a lower percentage of oxygen, less than normal at a higher percentage of oxygen," and "Oxygen has an influence antagonistic to chloroform."

Oxygen, therefore, increases the safety of chloroform administration, and in this apparatus it is always present in slight excess.

Oxygen also prevents cyanosis and gives a quiet, regular breathing, even with ether vapour, somewhat allied to that of a chloroform narcosis, and helps to abolish rigidity; it is also physiologically antagonistic to carbon dioxide, and enables the anaesthetist to use a high percentage of carbon dioxide as a respiratory stimulant in the early dangerous stage of deep narcosis, without cyanosis; it is also a powerful restorative when given in excess, and is always ready in case of emergency.

By the use of the rubber bag, full advantage may be taken of the powerful respiratory stimulant carbon dioxide, and oxygen apnoea is entirely eliminated.

The method enables the anaesthetist to use to the full the advantages of both ether and chloroform during an administration, and at the same time to modify and largely diminish the dangers of each. It gives an approximately definite percentage of ether and chloroform vapours throughout the administration. The administration is regular and continuous and the vapours are warm (76° F. on entering the bag). Oxygen is continuously administered in small excess, and is available in large excess at any moment in case of danger; its presence diminishes the danger of the administration; it causes quieter breathing and lessens rigidity, and it enables ether to be used more freely, and approximates the type of anaesthesia to that of chloroform.

The powerful respiratory stimulant carbon dioxide can be made full use of without inducing cyanosis, and oxygen apnoea is eliminated. The wide bore of the angle-piece relieves respiratory strain and its simplicity makes it easy to sterilize. The method is based on scientific principles and yet is essentially practical. Lastly, the above theoretical advantages are amply fulfilled in clinical experience. The method has been used in many hundreds of cases with success.

Mr. H. M. PAGE (Guy's Hospital) said: Dr. George Crile's method of anaesthetizing by means of nasal tubes was devised for preventing blood from entering the larynx in mouth operations, and for removing the anaesthetist well out of the way of the surgeon and his assistants, and for facilitating asepsis in operations on the neck by the patient's mouth and face being covered up. Dr. Crile devised this method for the use of ether, but I have for the last three years been in the habit of connecting the nasal tubes to a Vernon Harcourt inhaler, and thus giving chloroform where, from the presence of marked emphysema, bronchitis, and thickened vessels, I have considered this drug to be the better. In either case I use oxygen continuously, and have had an attachment added for this purpose, and when ether is being used I now have a coil of copper tubing running through a hot-water chamber, as in the instrument now made for me by Messrs. Down Bros. An opiate with the addition of atropine or scopolamine and atropine must be previously administered. The pharynx and upper part of the laryngeal opening is cocainized by a 2 per cent. solution when the patient is unconscious.

I induce anaesthesia by chloroform or a chloroform and ether mixture, as I find that I get more time for passing the tubes, packing, etc. I have also had a gag made for me which opens very wide, as I have found that the ordinary gag does not give sufficient room for packing comfortably in edentulous patients. In my experience the main point in this method is the packing of the pharynx. It must be done thoroughly, but with care not to compress the nasal tubes, special attention being given to the sides of the pharynx. How the packing is done is of more importance than the quantity used. I have found that a narrow sterilized gauze roll, 1½ to 2 inches, is the best for the purpose. I have had to repack the throat twice on account of faulty packing causing commencing cyanosis; one case I packed with my left hand owing to having a sore finger on my right, and on the other occasion I packed with a glove on—I am not accustomed to the use of india-rubber gloves. On one of these occasions our President was present.

The whole tongue can be removed by this method, but if the tonsil or pillars of the fauces are involved I prefer to use one of the other methods of anaesthesia, as the packing is inclined to get in the way of the surgeon. I have had some trouble from nasal obstruction, but have never failed to get over this difficulty either by using a smaller tube on one side, or more often by slightly dilating the passage. The patient recovers with great rapidity from the anaesthesia, always has a good coughing reflex, and is generally moving and almost conscious before leaving the operating table. In the large majority of cases shock is absent or very slight; in fact, I have only known one case in which decided shock was present, and this case made an uninterrupted recovery. The anaesthetist is altogether out of the way of the surgeon and his assistants when using this method.

My own cases and results in malignant disease are as follows:

	No. of Cases.
Excision of tongue, complete	2
,, half of tongue	5
,, half of tongue, part of lower jaw, and glands of neck	1
,, half of tongue, part of floor of mouth, and glands of neck	1
,, half of tongue and part of floor of mouth	3
,, malignant glands of the neck ...	15
,, epithelioma of lip and cheek ...	1
,, carcinoma of cheek	1
,, lower jaw (sarcoma)	1
,, malignant growth and fulguration in mouth and neck	3
	33

Of two of these cases I have only made a note that they left the theatre in very good condition and have not noted the ultimate result; but, personally, I am quite sure that they recovered, as I have followed all my cases and have no knowledge of a death. All the rest of the cases made an uninterrupted recovery, except one case in which death occurred suddenly on the fifth day after operation from cerebral hemorrhage. There was no lung trouble. The common carotid artery had been tied.

I am trying to trace the two mentioned above.

A good many other cases besides mine have been operated on at Guy's Hospital under this method of anaesthesia, but I have only been able to find one death recorded as following the use of Crile's tubes. The case was not mine, and I know nothing of the condition of the patient before operation. There was nasal stenosis, and only one tube was used, which appeared to give sufficient air. The whole tongue, part of the floor of the mouth, and the glands on both sides of the neck were removed at one sitting. The patient died a few days after, from bronchitis. Ether was the anaesthetic employed. Dr. H. R. Phillips has kindly

allowed me to include the cases of malignant disease in which he has used this method, which are as follows:

	No. of Cases.
Excision of part of tongue	1
,, part of tongue and glands of neck ...	2
,, part of tongue and part of lower jaw	2
Scraping malignant disease of lower jaw ...	2
	7

All of these cases made an uninterrupted recovery.

Both Dr. Phillips and I have used this method in other cases, but I have confined my remarks to malignant disease. Finally, I would state that in suitable cases I prefer this method to any other in dealing with malignant disease of the mouth and neck, both from the point of view of the patient and the surgeon, and also because the anaesthetist, once the packing is properly done, has no fear of the haemorrhage, nor is he troubled by reflex effects, and shock, as a general rule, is absent or very slight.

The PRESIDENT (Dr. Dudley Buxton) was glad to say that Mr. Leedham-Green had dealt out even justice as between analgesia and anaesthesia. Statistics were not always reliable, and everybody was apt to think his own method was the best, but the really best method was the one which worked out best in the hands of others, skilled and unskilled. All the figures upon chloroform mortality were vitiated by including administrations by the inexperienced. On the other hand, the figures given of spinal analgesia mortality were those of experts, and the comparison between the two was hardly fair. Another factor was that a dosimetric method was used in spinal analgesia, whereas the chloroform method employed was comparatively seldom dosimetric. The Dubois and Vernon Harcourt apparatus had no deaths. The Roth-Dräger had some deaths because, although the apparatus was dosimetric, it was capable of giving very high percentage vapours of chloroform, and hence gave no substantial safeguards against overdosage. He did not agree with Mr. Leedham-Green's statement that there were no deaths in regional analgesia—cases had been recorded in which its employment had certainly contributed towards the fatality, although the result was attributed to that curious condition termed "status lymphaticus." Emotional and psychological reasons called for general anaesthesia, and indicated also the employment of a preliminary narcotic. In reference to remarks of Dr. Ehrenfried and Mr. Kelly, he thought a good deal of practice in the intratracheal method was necessary before anaesthetists attempted it on the human subject. He was inclined to think that pulmonary after-effects from intravenous ether were less than after inhalation. Rectal etherization, to which Mr. Leedham-Green had referred, had been introduced in London by the speaker twenty years ago, and his apparatus had been apparently overlooked and his results ignored, for practically identical apparatus had been reinvented and published in medical papers in other countries under the names of the gentlemen who had adopted the practice. The after-effects attributed to ether when that anaesthetic was employed in the inhalational methods were extraordinarily good, when it was remembered that the anaesthetic was commonly used in a reckless way. Deaths had been recorded in the United States, but these were clearly due to failure of the heart arising through furious struggling, incident in some cases to faulty methods of administration. Post-operative ether sequelae were more numerous, but even these had to be carefully scrutinized before they were shown to be the sole result of ether. As he had pointed out in opening a discussion before the Society of Anaesthetists in 1900, the so-called ether pneumonia was a common catarrhal condition due to neglect of the commonest rules of surgical hygiene, or, as Hoelscher had demonstrated, to aspiration of buccal mucus and condensed ether vapour into the air passages. When atropinized a patient was immune from this danger. The main advantage of the modernized "open" method of etherization was that it gave the patient a less concentrated vapour of ether, and, as a result, he was saved from ether toxaemia. The skilled anaesthetist could safeguard his patient from most of the after-dangers of general anaesthetics, but, unfortunately, no skill could safeguard patients for whom local or spinal analgesia had been employed, from the psychic shock which, as Crile had shown, was so prejudicial. Of course, persons differed widely in this regard, but whether they revealed their sensations or not, it had been pretty clearly shown by experiment that the central nervous system was not protected from damage by the use of analgesics. The after-effects of the insufflation of ether by the intratracheal method were as yet unknown, as the number of cases recorded were comparatively few. So far it promised well in suitable cases and in the hands of experts. He congratulated Dr. Ehrenfried and Mr. Kelly upon their apparatuses, which were certainly less cumbersome than the older forms.

Mr. LEEDHAM-GREEN, in reply, said he still doubted the advantages of the intravenous method. He was much interested in the apparatus shown by Drs. Kelly and Ehrenfried, and thought them very useful for severer operations on the lungs. He did not think the passage of a large catheter into the trachea an unirritating procedure. He was afraid that its long-continued presence in the trachea would militate against its frequent use in ordinary head and neck operations. He agreed that the noise of the electric motor was disturbing.

DISCUSSION ON THE USE OF ALKALOIDAL BODIES PRIOR TO INHALATION.

OPENING PAPER.

By FRANCIS W. BAILEY, M.R.C.S., L.R.C.P.,
Anaesthetist, Royal Infirmary, Liverpool, and Liverpool Dental Hospital.

I PROPOSE that we should ask ourselves in the first place, Why is it we as anaesthetists have called in the aid of drugs, or a combination of drugs, to assist us in producing a state of anaesthesia other than that which is usually obtained by inhaling the vapour of ether, chloroform, ethyl chloride, or nitrous oxide gas, or by methods which obtain more rarely in this country—namely, infusion or subdural injection anaesthesia?

Is it not because we not only in this country but in all countries are tending to place the administration of anaesthetics in the hands of specially appointed persons more often than heretofore, and that these individuals are always straining for perfection? Let it not be imagined that it is because we are looking out for something new, or for a desire for experimenting, because the field for this is somewhat limited in the branch of medicine which we are practising. Perhaps it may be with a view to abrogating the dissatisfaction which arises with regard to anaesthetic methods in some clinics, and we welcome any little help, which is better than none.

Or, let me ask you once more, Is it a reversion to type? is it a revival of narcotic fashion? For a reply turn over the pages of England's immortal bard, and there we find that Shakespeare makes frequent allusion to anaesthetic draughts, and further back again, Homer mentions the anaesthetic effects of nepenthe; but we must not dwell upon the past—the present and the possibilities of the future are before us.

The narcotic effects of opium were well known to the older surgeons of pre-anaesthetic days, and it seems to me that the technique of the present-day anaesthetist is a combination of the methods adopted before the discovery of anaesthesia with those methods and means elaborated since the discovery of anaesthesia, both tending to make a component whole which some day may lead us to our ideal.

Most, I might almost say the majority, of my work in the administration of anaesthetics in conjunction with the introduction of alkaloidal bodies into the system by hypodermic or other means, has been done with a combination of atropine and morphine, or atropine alone—drugs which, in my opinion, are safer to use than many of the proprietary preparations whose virtues are extolled by the manufacturing chemist for the clinician's use. That which may be proved to be

experimentally true in the laboratory does not always appear to be advantageous to the anaesthetist or clinical practitioner.

We know well the virtues and influences of our old and tried friends atropine and morphine, and to these may be added the the hyoscine preparation known as scopolamine. It is to these I would apply the term "alkaloidal bodies"; these should be the drugs of our choice. With regard to atropine, we must remember that the three great vital centres are markedly affected by its administration. Briefly, the respiratory centre is powerfully stimulated both in frequency and extent of movements, subsequently it is paralysed by poisonous doses. One of the most striking actions of atropine is paralysis of the peripheral terminations of the vagus in the heart; in other words, the abolition of the inhibitory action of the vagus. To what extent this action of atropine occurs, if it occurs at all in the doses usually given previous to the administration of an anaesthetic, I am not prepared to say, but this I will say, we should not lose sight of the possibilities of its action being present after administration, as shown, it may be, by the rise of the pulse-rate to, it may be, twice its previous rate after a full dose.

Another point with regard to the atropinized vagus is that the terminations of this nerve in walls of the bronchi are paralysed; thus the tension of the muscular coats of the bronchi is diminished, and as a result the air current is facilitated. I shall have more to say upon this later on. The vasomotor centre for a time is stimulated and then depressed; the systemic arteries are first contracted and the blood pressure raised; subsequently the vessels are relaxed and the pressure lowered. Lastly, atropine paralyses the chorda tympani in the submaxillary gland, but remember that sympathetic secretion can be obtained, the sympathetic remaining unaffected and the vessels of the gland dilate as usual under stimulation.

With regard to the action of morphine, I do not intend to elaborate upon the action of this well-known drug, except to say that morphine slows the pulse-rate, impairs respiratory movements, and renders respiration shallow and acts as a vaso-dilator, but these effects are only pronounced in large doses.

With regard to scopolamine, we get similar action to that of atropine with large doses.

I must crave indulgence for having traversed facts well known to all, but I wished to present a vivid picture of what we are doing to the human frame by introducing drugs previous to anaesthesia. We are not anaesthetizing a normal patient, so to speak, but one more or less in a narcotic state, and one not answering, it may be, to all the laws appertaining to the physiology of the human subject. Just imagine you are driving a motor car in which the brakes are inoperative, and the only means of stopping the car is by running off or shutting off the petrol supply—there is danger ahead which you may not easily control. One most important decision is the drug or drugs to be selected and the dose to the administered, and whether it should be full, medium, or small, we must always bear in mind that there is such a condition as idiosyncrasy, a drug tolerance; cases must be known to all present to-day of, for example, belladonna poisoning occuring even after the application to the skin of the liniment, and if such a reaction will take place in a patient in a non-anaesthetic condition, we must ever be aware of the possibility of it occurring, it may be, with more alarming symptoms to a patient in an anaesthetic condition, suffering, it may be, from debilitating disease or surgical shock. As regards the dose given, age incidence counts for much; clinically we know that atropine is well borne by children, but more care has to be exercised in the exhibition of morphine, and I doubt very much the wisdom of using the latter drug previous to anaesthesia in the case of children. I am an advocate for what I would call a moderate dose, because I believe alarming symptoms may arise during anaesthesia from overdrugging; these I will mention when considering the disadvantages of the method.

With regard to scopolamine, there are some—and perhaps it may be rightly so too—who see some use in this drug as a preliminary; but may we not obtain all we want by the use of atropine alone or in combination with morphine? It is doubtful whether polypharmacy is to be encouraged previous to the induction of anaesthesia. I have always thought the simpler the methods and apparatus we use in anaesthesia, the greater the safety of the patient and the less the difficulties of the administrator.

Let me now put before you a few of the advantages of the preliminary administration of an alkaloidal body previous to the induction of anaesthesia; but we must not lose sight of the fact that there are also disadvantages.

First, it undoubtedly economizes the amount of ether and chloroform which is used in some cases, but not in all, but this, I take it, as professional men we should not consider. In cases of intradural anaesthesia when consciousness is not abolished it is of decided use. In nervous patients and alcoholic individuals, who are usually the most difficult to deal with at the induction stage, we should advocate its use. In these cases, too, when the course of the anaesthesia might not be uniform or smooth, it certainly assists in the direction we desire—a quiet and even anaesthesia. Some patients are prone to salivate, others will not, in spite of the absence or presence of the administration of atropine; but in cases when we can atropinize a patient previous to administration, there is no doubt that there is an advantage in a preliminary dose of an alkaloidal body. I always like to hear a patient say, "I am thirsty," or "My mouth is dry," on coming into the anaesthetic room, for then I am sure I am dealing with a patient in an atropinized condition, and then I hope to see diminished secretion of saliva, and consequently diminished risk of lung complications, which is brought about by saliva and mucus *plus* ether trickling down the trachea into the bronchi. In some cases, too, there is an advantage in a preliminary soporific effect being obtained—for example, cases in which there might be pain after an operation, or pronounced excitement sometimes amounting to delirium, which might not have happened had there been a previous administration of atropine or morphine. One extremely important advantage obtained is the decrease of post-operative vomiting. So much for the advantages; I will now recapitulate some of the disadvantages.

The preliminary administration of an alkaloidal body may and does sometimes fail to produce the desired effect. In the case of old people, who usually are easily anaesthetized and take very little anaesthetic, and those with calcified cartilages or emphysematous chests, it appears to me doubtful whether it is advantageous to give atropine—and certainly I would not advocate morphine; the respiratory movements are impaired, and I have noticed on some occasions respirations of a Cheyne-Stokes character—I take it, due to a depressed centre being stimulated from time to time by an accumulation of asphyxial blood. It may be found that it is necessary sometimes to give an anaesthetic to patients suffering from renal trouble—it might be albuminuria in a marked degree; in my opinion it would be inadvisable to administer an alkaloidal body of any description because, should a diminished output of renal secretion occur, with an alkaloidal body *plus* ether, chloroform, etc., we should be only increasing the possibility of, so to speak, putting the kidney machinery out of gear. I think this is a point we should like to hear discussed. As to the wisdom of exhibiting morphine in bronchitis, this, too, is debatable, for we should depress respiration if we give it in doses which would produce any perceptible systemic action. Respiratory depression is also liable to take place in anaemic subjects or those suffering from a wasting disease such as tuberculosis, which might require operative interference. It must be remembered that the combined action of these drugs is much greater than the sum of their separate actions could be, so that if any unfortunate reaction is produced we do not know what the precise cause may be; you can easily realize that a full dose of scopolamine-morphine administration might cause death from paralysis of respiration, and that artificial respiration and all means of stimulation would be ineffective. So that there are contraindications in all conditions whereby the respiratory centre is depressed, or likely to be depressed through prolonged anaesthesia, or indirectly by shock or haemorrhage or severe cardiac lesions.

It is almost impossible to foresee all the conditions which may be caused by deviation from the normal; but when any of them are there and we have injected an alkaloidal body it is beyond our further control, and every additional narcotic complicates anaesthesia. Scopolamine, or, for that matter, morphine and atropine, do not help us much in producing muscular relaxation in either intra-

dural or intravenous anaesthesia. It is just doubtful, in my mind, as to whether we are wise in aiming at producing a long after-sleep, and so interfering with expulsive expiratory efforts which may take place during returning consciousness. Are we not helping, by keeping the patient quiet and in a recumbent position, to produce pulmonary oedema? The thirst and dryness of the throat produced in some patients by atropine are very distressing, and produce discomfort—a condition of affairs very undesirable in the case of stomach or bowel operations. The question as to whether we may produce fatty degeneration in some organs of the body I leave to other members of this Section to discuss. Although I cannot speak definitely, I should say its rarity eliminates any gravity which may be attached to it. In the case of intradural anaesthesia the previous administration of strychnine sulphate, it is admitted, I believe, overcomes the dangers of respiratory paralysis.

I do not advocate complete narcosis by alkaloidal drugs previous to anaesthesia produced by one or other methods, with a view to doing away with the fright or positive terror of operations which few if any patients have in the present day, because I have so rarely come across, in all my experience, a patient who cannot be reassured, or whose confidence cannot be obtained previously by the surgeon or the anaesthetist.

If we aim at complete narcosis we must give large doses, say, $\frac{1}{50}$ grain of atrophine, or a $\frac{1}{4}$ grain of morphine, and I have come across cases where one used to give what I might call heroic doses, and where we used to get respiratory embarrassment, causing discomfort to the patient and anxiety to the anaesthetist. When big doses are given it is difficult sometimes for the anaesthetist to tell whether the patient is under the anaesthetic or under the drug administered. So that I would suggest not any larger quantity in the case of atropine than will be sufficient to diminish vagus action. We find it difficult sometimes to abolish reflex effects altogether during anaesthesia; this is to be noticed in deep gall-stone operations, chiefly bearing on respiration, when grunting expirations may occur.

As for its assistance in rapid induction, I think a penalty is to be paid, so to speak, in that there is some disturbance in the smoothness of the subsequent anaesthesia.

With regard to the routine administration of alkaloidal bodies previous to ordinary anaesthesa in all cases, I think I have placed before you sufficient facts to prove that there are certain contraindications; there are cases in which no advantage is gained and some cases in which it may be a disadvantage.

As to the technique of administration, the desired effect will not be obtained unless the drug is given, say, half an hour to three-quarters of an hour previous to operation; this is the ideal, but it is not always possible in an active clinic; the dose should be moderate—$\frac{1}{150}$ grain of atropine, $\frac{1}{6}$ grain of morphine, and $\frac{1}{100}$ grain scopolamine.

In conclusion, let me state that the administration of alkaloidal bodies previous to anaesthesia is a welcome addition to the anaesthetist's armamentarium in most cases. Some advantages we do admittedly derive from their injection; such are: diminished salivary secretion at the induction stage, a quiet and even anaesthesia at the complete stage, and diminished vomiting on returning consciousness; still, many of these can be obtained without them by expertness on the part of the anaesthetist and by judicious choice of the anaesthetic given.

DISCUSSION.

Mr. G. SECCOMBE HETT (London) said: I propose to discuss this matter mainly in its practical aspects and from the point of view of a surgeon engaged in operations upon the nose and throat. In my practice one may say that in all, or practically all, operations alkaloids take a part in the production of anaesthesia. Of these, of course, cocaine is the chief and the one most frequently used, either alone or in combination with one or more of the others. Many operations, such as submucous resections and operations on the nasal sinuses, are performed under local anaesthesia, and in these I have been struck by the advantage to the patient of a preliminary injection of a combination of the narcotic alkaloids. It avoids mental depression during the operation, and, whereas formerly many patients who seemed to be recovering well from the operation, fell after the lapse of a few days into a condition which I may term "traumatic neurasthenia," I now find, since using a preliminary injection of alkaloids, that this condition is rare. I like the injection to be given to the patient on the operating table, and the nasal packing to be done at the same time. He is then left to rest quietly, and I frequently find him asleep on my arrival to operate. I have never seen this injection followed by any respiratory failure. Some of my operations are conveniently performed under ether given intravenously, and in these the preliminary injection of alkaloids is of great value. I regard atropine rather as an aid to the surgeon than as a narcotic; in enucleation of tonsils it is very useful in abolishing reflexes, which are apt to ensue in the course of this operation. I refer to laryngeal spasm and inhibition of the heart. On the other hand, I do not think the administration of scopomorphine advisable in these cases or in operations on adenoids. No undesirable after-effects have followed the use of any of these alkaloids in my practice.

Dr. G. A. H. BARTON (Hampstead General Hospital) said: Owing to lack of time I have been unable to make a complete analysis of cases in which I have used scopolamine and morphine as a preliminary to anaesthesia. My remarks will therefore be brief and general. The drugs mentioned have been the ones chiefly employed. I have occasionally used atropine, either in addition to or in place of one or both of these drugs. I believe it has little or no narcotic effect, and that scopolamine is equally effectual in preventing secretion of mucus. I started the use of these drugs some three years ago in a tentative way, first in abdominal cases in order to get more relaxation whilst using open ether as the anaesthetic. I understand that some surgeons consider they have an opposite effect or tend to cause rigidity, but this is quite contrary to my experience once the patient is really anaesthetized, though I am willing to admit that in some cases during induction and first few minutes of anaesthesia there has been a good deal of rigidity—not more, however, than there frequently is during the early stages of simple ether anaesthetization. Later I took to using scopomorphine as a preliminary to thyroidectomy and the use of the endoscope by the oral route, in order to diminish the secretion of mucus and the troublesome reflex cough so frequently present. So satisfied was I with the advantages of the method that I soon adopted it as a routine in all surgical operations on adults, provided no contraindications were present. The drugs used have been either Burroughs and Wellcome's tabloids of morphine sulphate and hyoscine hydrobromide or Riedel's scopomorphine, and I cannot say that I have observed much difference in action between them. The most suitable time for injection I find to be one hour before operation, though, of course, it has not always been possible to secure anything like accuracy in this in hospital work. The dose has generally been left to the discretion of the house-surgeon, usually $\frac{1}{100}$ gr. of scopolamine and $\frac{1}{4}$ gr. or $\frac{1}{6}$ gr. of morphine. The question of dosage is an important and difficult one. In dealing with these powerful alkaloids one has to be cautious. I have been impressed by the variable condition of patients on their arrival in the theatre; with the same dose some will be very drowsy, others apparently hardly affected. Every patient would seem to have his own appropriate dose, which unfortunately, in the majority of cases, one can only guess at. I well remember years ago having to anaesthetize a lady whom I knew well. It was before these preliminary injections came into vogue. She had been suffering for several weeks from an extremely painful malady, but, being a diabetic, operation had been delayed. To relieve her pain morphine had been given up to doses of 1 gr. hypodermically, but on this losing its effect, hyoscine was given, and at the time of operation I was aware that $\frac{1}{200}$ gr. of hyoscine would send her soundly to sleep in a very few minutes. I had therefore no difficulty in dealing with this case. Scarcely any ether was required, and the results were in every way perfect. I might here mention that the effects of hyoscine or scopolamine, as it is variously called, are much more rapidly produced than those of morphine and pass off more quickly.

In the vast majority of cases patients arrive on the operating table drowsy and placid. The operation has lost its terror for them. The induction of anaesthesia is generally rapid and devoid of struggling. Here and there, of course, one comes across an individual upon whom morphine acts as an excitant, and then there is perhaps much struggling, with rigidity and delayed induction. During anaesthesia I have particularly noticed two points: one is the almost entire absence of shock during the severest operations, and the other is the placid, quiet breathing, with absence of all coughing, retching, or straining, which obtains with even the lightest anaesthesia accompanied by a brisk corneal reflex. A great advantage of the method is that it enables good anaesthetic results to be obtained with a minimum quantity of the safe open ether. I have frequently used less than 4 oz. in operations lasting an hour. I practically now never give chloroform alone except in a Junker's inhaler, and not often then.

Owing to the small quantity of anaesthetic used, the after-effects are generally negligible. The patient frequently sleeps peacefully but not too heavily, and has all his reflexes present. There is seldom any vomiting on awaking; when it does occur, I have found it more frequently on the day following the operation. With an experience now of some hundreds of cases, I cannot recall any in which any deleterious effects attributable to the alkaloids have appeared either before, during, or after operation. As to contraindications, I do not think they are many. I should avoid the method in children and some old and feeble subjects, also in those with kidney disease or advanced disease of the lungs. Owing to the respiratory depression caused, I have so far not used it in operations involving haemorrhage into the air passages. Of omnopon I have no experience. Theoretically there seems to me no advantage in giving all the alkaloids of opium, many of which are deleterious without being narcotic.

Mr. VICTOR FIELDEN (Royal Victoria Hospital, Belfast) referred to the use of cocaine, morphine, scopolamine, and atropine. His use of cocaine was confined to operations upon the eye, nose, and throat. For iridectomies and extractions of cataract the preliminary instillation of a drop of solution of cocaine into the conjunctival sac was of great utility in avoidance of such a deep degree of anaesthesia, whilst for nasal cases it was equally efficacious. Morphine, atropine, and scopolamine, alone or combined, were of great service in the vast majority of cases during the operation by diminishing the amount of anaesthetics required, and later by prolonging the sleep, with diminution of after-effects; but the difficulty one met with was the impossibility of diagnosing beforehand those cases which could not stand morphine, etc., even in small doses. The weakening of the respiration was demonstrated when the Harcourt inhaler was in use by the diminished movement of the inspiratory valves, requiring watchfulness on the part of the anaesthetist. With ether, morphine and atropine possessed the advantage of diminishing greatly or completely the secretion of mucus in the respiratory tract.

Dr. W. J. MCCARDIE (General Hospital, Birmingham) said: According to Honigmann's dictum, two drugs which act in the same direction accentuate the effect of each other, and smaller doses of each are needed to produce the equivalent effect of one of them. Again, the combination of scopolamine and morphine with an inhalation anaesthetic much lessens the dose of all three drugs needed to produce a given effect, so that the total toxic effect of anaesthesia is much diminished. This is a great advantage of combining alkaloids with a general anaesthetic. Another very valuable effect is the production of *dryness* of the mucous membranes of the respiratory and alimentary tracts by which respiratory and stomachic after-effects are greatly lessened. What is the best proportion in which to combine scopolamine and morphine? Schneiderlin's and Korff's proportion is $\frac{1}{100}$ to $\frac{1}{4}$ grain, and this still seems to be the standard proportion. For myself, I think that less morphine is often advisable, because it depresses respiration so much, though if ether is being given this "damping down" effect is generally advantageous. My own standard dose is $\frac{1}{200}$ to $\frac{1}{3}$ grain, and I rarely exceed $\frac{1}{100}$ to $\frac{1}{6}$ grain. The determination, experimentally, of the best proportion of scopolamine and morphine would be very useful, especially such a proportion as would not cause marked enfeeblement of respiration. I have used hyoscine ($\frac{1}{200}$ grain) combined with liq. strychninae (m. v) in several cases where I have feared the effect of morphine, and have found generally a sufficiently calmative action, and that the patient's colour was florid and his respiration stimulated. The free venous dilatation and consequent oozing caused by scopolamine and morphine is a serious drawback to its use in delicate operations—for example, eye operations and those on the nasal septum. The use of these sedative drugs enables the operator to begin earlier and the anaesthetist to stop the administration sooner.

Mr. C. CARTER BRAINE (Charing Cross Hospital) said: On the whole I have been favourably impressed with the previous injection of atropine, morphine, and scopolamine. In my practice I have not used more than $\frac{1}{6}$ grain of morphine, and I consider that there is a tendency to give too much of this drug. It is very difficult to recognize the difference between anaesthesia and slumber when these alkaloidal bodies are used, and this difficulty is fully recognized when teaching students to administer anaesthetics; they are very liable to permit the patient to pass from anaesthesia to sleep, and in the absence of all eye symptoms wherewith to rectify this error I find it difficult to point out when this change is taking place. The anaesthetist should always be informed when any of these alkaloids have been injected prior to his advent, and should not be left to find out the fact for himself by the behaviour of the patient under the anaesthetic. In some instances I have thought that the action of morphine has been increased by the anaesthetic, and I should like to know whether this has been the experience of any of my colleagues.

Dr. A. BERESFORD KINGSFORD (University College Hospital) said: I always dislike the use of cocaine in cases for which chloroform is likely to be wanted, because of the depression frequently seen when this combination of drugs is employed. With regard to the hypodermic injection of morphine and scopolamine some time before operation, I find the effects vary greatly in different cases. Some patients are very sleepy, others are quite wide-awake, but all seem comfortable. I regard this, therefore, as a merciful procedure, very suitable for nervous people. Some patients require but little anaesthetic after the hypodermic injection, but in other cases not only may ether have to be given as liberally as possible, but recourse to chloroform may be necessary for a time in order to secure full abdominal relaxation, as, for instance, in cases of Wertheim's operation. Then some cyanosis is prone to appear, in spite of a good airway and some administration of oxygen. If the oxygen is given more freely in these cases there is some tendency to arrest of respiration—especially when the character of the surgical procedures suddenly alters—an event which is inconvenient and does not look well, though it is, perhaps, never alarming to the anaesthetist. A minor drawback of the administration of morphine and scopolamine is that it increases the difficulty of demonstrating the stages of anaesthesia to students, on account of the sluggishness of pupils, etc., thereby entailed.

Dr. WM. FINGLAND (Liverpool Royal Infirmary) said: For my part, I deprecate the preliminary injection of alkaloids as an adjuvant to anaesthesia *in toto*, and consider their use a reproach to the anaesthetist, who should be able to accomplish his purpose quite easily without their aid. There is no objection, perhaps, to the administration occasionally of atropine for the sake of its drying effect on mucous membrane, and morphine may sometimes be advisable in abdominal operations for the sake of the post-operative ease and quiet it ensures.

The PRESIDENT said: I cannot see eye to eye with Mr. Fingland as regards this method, for my experience has convinced me of its great value. It lessens the amount of anaesthetic required, and so diminishes the liability to after-effects. It is also very valuable in the case of extremely nervous people, a type that I still find sufficiently abundant in London. After administering the alkaloids, it is of the utmost importance to lay stress on the necessity

for absolute quietude in the patient's environment until the time comes for the administration of the anaesthetic. Scopolamine has been alleged by some to be a dangerous drug, and it has been said that the combined action of the alkaloids is greater than the sum of their separate actions. With this I do not quite agree. In some directions they work together, but in others they antagonize, and I believe that in respect of many of their deleterious effects they are antagonistic. All three together form a most valued combination. When any one of the three is omitted the effects for good are lessened, while the power for evil is increased. It is essential that each patient's condition should be studied and the appropriate combination and dose of drug decided upon. As the question of the advisability of using cocaine in conjunction with chloroform has been raised, I would remind the meeting that Brown-Séquard proved the liability to inhibition of the heart through strong vapours—for example, chloroform impinging upon the mucous membrane of the nose—and that cocaine used by Obalinski's method previous to the administration is believed to abolish this nasal reflex by blocking the path of afferent stimuli. No doubt, however, the best plan is to avoid such perilously strong chloroform vapour.

Mr. BAILEY, in reply, said he thought the discussion had brought out the fact that anaesthetists were on the right lines in using these preliminary injections. There still, however, seemed to be difference of opinion as to dosage generally and considerable difficulty in arriving at the suitable dosage for individual patients.

THE EFFECT OF CHLOROFORM AND ETHER ON THE LIVER AND KIDNEYS IN HEALTH, AND ITS SIGNIFICANCE IN CERTAIN INFECTIVE CONDITIONS.

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DURING the past two years I have had an opportunity of seeing two fatal cases of post-anaesthetic acid intoxication and a number of other cases in which such a condition has, in my opinion, been largely instrumental, or assisted in conjunction with some other malady, in bringing about a fatal termination.

Many fatal cases have been reported in recent years; but when the total of anaesthetics administered daily is taken into consideration the number is exceedingly small, even allowing for those cases where the characteristic symptoms may be masked and the condition undiagnosed. But between these fatal cases of post-anaesthetic acidosis and those in which no symptoms of acidosis appear there are an enormous number in which symptoms more or less severe show themselves and retard the patient's recovery, and some in which they assist, in combination with a weak heart or the shock of a severe operation, in causing death.

Guthrie, in his original paper in 1893 on acid intoxication following the administration of chloroform, believed that the metabolic functions of the liver were upset by some pre-existent disease, and that the anaesthetic acting as a poison was the "last straw," causing the onset of an acid intoxication. That this pre-existent disease is present in the shape of some acute infection—an acute appendicitis being the most common—in a large proportion of reported fatal cases will be shown by an investigation of the recent literature on the subject.

Beesly in 1906 proved that in every acute infective condition there was an accompanying acute acetonuria. This acetonuria is but an indication of an interference with the normal functions of the liver, particularly the metabolism of fats. He also showed that it was in those cases in which there was an acute acetonuria that symptoms of acidosis were likely to appear after an administration of chloroform; so that, in acute infective conditions at least, we have Guthrie's condition fulfilled—that is, that the liver cells are already damaged when the chloroform is administered and the patient predisposed to acid intoxication.

I have sections of the liver of a patient who died of a general streptococcal infection, the fatty changes in which are almost indistinguishable from those produced by chloroform.

It should be borne in mind that the amount of acetone in the urine is no criterion of the amount of damage done to the liver either by some infection or by the anaesthetic, as its elimination may be defective, owing, possibly, to damage done to the renal epithelium as well as to the liver cells, so that a small amount of acetone in the urine may indicate much damage done with defective excretion of acetone, while a large amount of acetonuria may mean little damage done but rapid elimination of the acetone.

The experimental work done for this paper had for its object the determination of the extent and nature of the injury done to the cells of the liver and kidneys, by an acute infection, by chloroform and ether, the significance of a combination of the two, and finally a short series of experiments were done with the object of showing the value of glucose in combating the symptoms caused by such charges.

After the first series of animals, experiments with ether were discontinued, as the results obtained were almost negative and merely bore out work which had already been very fully done.

In the first series of experiments (Table I) twenty-one large healthy rabbits were used. Chloroform in doses varying from ½ c.cm. up to 4 c.cm. was given subcutaneously, and in arriving at conclusions the proportion between the weight of the animal and the dose given was taken into consideration.

TABLE I.—*Injections of Chloroform and Ether.*

No.	Dose.	Date of Death.	Liver.		Kidney.
			Necrosis.	Fatty Degeneration.	Fatty Degeneration.
1	½ c.cm. chloroform	Killed 3rd day	–	+	Slight.
2	½ c.cm. chloroform	Killed 5th day	+	+	Slight
3	1 c.cm. chloroform	Died 2nd day	+	+	+
4	1 c.cm. chloroform	Died 3rd day	+	+	+
5	1 c.cm. chloroform	Killed 4th day	+	+	+
6	1 c.cm. chloroform	Killed 5th day	+	+	+
7	1 c.cm. chloroform	Died 2nd day	–	+	+
8	1 c.cm. chloroform	Died 2nd day	+	+	+
9	1 c.cm. chloroform	Died 1st day	–	+	+
10	1 c.cm. chloroform	Killed 4th day	+	+	+
11	1.5 c.cm. chloroform	Died 2nd day	+	+	+
12	1.5 c.cm. chloroform	Died 4th day	+	+	+
13	2 c.cm. chloroform	Died 2nd day	–	+	+
14	2 c.cm. chloroform	Killed 4th day	+	+	+
15	2 c.cm. chloroform	Died 3rd day	+	+	+
16	2 c.cm. chloroform	Died 3rd day	+	+	+
17	2 c.cm. chloroform	Died 1st day	–	Slight	Slight.
18	3 c.cm. chloroform	Killed 5th day	+	+	Slight.
19	3 c.cm. chloroform	Died 2nd day	+	+	+
20	4 c.cm. chloroform	Killed 5th day	+	+	Slight.
21	4 c.cm. chloroform	Killed 5th day	+	+	+
1	3 c.cm. and 4 c.cm. ether	Killed 1st day after 2nd dose	–	–	–
2	4 c.cm. and 4 c.cm. ether	Killed 4th day after 2nd dose	–	Very slight	Slight.
3	5 c.cm. and 6 c.cm. ether	Killed 1st day after 2nd dose	–	–	–
4	6 c.cm. and 6 c.cm. ether	Killed 4th day after 2nd dose	–	Slight	Slight.

The train of symptoms following the injection was the same in all cases. Anaesthesia varying from a mere drowsiness to deep narcosis lasting as long as forty minutes came on after each injection, the animals recovering completely in two to three hours, and taking their food as usual. In about twenty-five to forty-eight hours after the injection they began to get drowsy, and twelve out of the

twenty-one died in from forty-eight to ninety hours. The remaining animals recovered from this drowsiness and were killed about twenty-four hours later, when they were well on the road to recovery.

In every case the animals lost weight fairly rapidly, one weighing 2,110 grams losing as much as 250 grams in forty-eight hours; ½ c.cm. of chloroform did not appear to be a lethal dose in animals of over 1,000 grams weight, neither of those receiving this dose showing any serious symptoms of subsequent intoxication, whereas five out of eight given 1 c.cm. and four out of five given 2 c.cm. died. It is curious that only one animal given 3 c.cm. or over out of four died.

Chloroform prepared from acetone, methyl alcohol and ethyl alcohol was used, but no definite difference was noted in their actions. Some of the animals were killed with chloroform and the remainder by a blow on the head. The liver and kidneys were removed at once in each case.

Macroscopically, the liver in most of the animals appeared pale, and in some the patches of fatty degeneration and necrosis could be plainly seen. The kidney appeared normal. The stomach was distended with freshly ingested food in every case.

Microscopically, frozen and paraffin sections were cut of the liver and kidneys of every animal, the former being stained with haematoxylin and Sudan III or Scharlach R., and the paraffin sections with haematoxylin and eosin or haematoxylin alone.

In the liver there was marked fatty degeneration in every specimen and in many definite necrosis at the centre of the lobules. The typical picture was as follows: A few healthy cells could be seen immediately around the central vein of the lobule, and outside this, occupying the centre of the lobule, the cells appeared necrosed and the normal structure completely altered, the cells being broken and the nuclei often irregular. Many fat globules were to be seen in the necrosed area. The middle third of the lobule was occupied by cells loaded with fat globules of varying size, the larger droplets being towards the periphery; outside this again were comparatively healthy cells, forming the peripheral zone of the lobule, though these cells nearest the fatty zone were very granular, and in the paraffin sections were vacuolated, the granules seeming to be very minute fat droplets. In some few specimens there was no necrosis, and the fat then occupied the centre of the lobule, while in others there was little fat and the lobule was almost entirely necrosed, the fatty change seeming to be a precursor of the necrotic changes.

In the kidneys there were definite pathological changes in every specimen. Nearly all showed marked fatty degeneration in the epithelium of the loops of Henle, and in some this was also present in the convoluted and collecting tubules. In other specimens the epithelium of the tubules was much swollen in the frozen sections, presenting all the appearance of well-marked cloudy swelling, while the paraffin sections of the same specimens showed the epithelium to be minutely vacuolated, particularly in the loops of Henle, the part of the kidney to be first and most severely injured. In no specimens were the glomeruli at all affected. The amount of chloroform given did not bear any definite relationship to the amount of damage done, the most marked changes seeming to be produced, however, by doses of about 1 c.cm. per 1,000 grams of the animal's weight.

In addition to the above, four rabbits of practically the same weight (Table I) were given respectively 3, 4, 5, and 6 c.cm. of *ether* subcutaneously. After recovering from the initial anaesthesia they appeared perfectly well and remained so. Three days later they were given a second dose. Again they completely recovered from the anaesthesia, and remained perfectly well, in fact they all gained a little weight. The day after the second injection two of the animals were killed, and the other two four days later.

Macroscopically, the liver and kidneys appeared normal.

Microscopically.—In the liver there were a few fat globules seen towards the centre of the lobules in two specimens, and in the paraffin sections of those a few of the cells were a little vacuolated. There was some congestion. The changes, however, were insignificant in comparison with the chloroform specimens. In no specimen was there any necrosis.

In the kidney a few minute globules of fat were seen in the epithelial cells of the convoluted tubules and loops of Henle, and in these specimens there were a few minute vacuoles in the paraffin sections in the same position. There was cloudy swelling present.

In the second series of experiments eight animals were used. Six rabbits were given chloroform by inhalation and two ether. Of those given chloroform, one was given half an hour of anaesthesia, one an hour, and the remainder half an hour daily for six, ten, thirteen, and fourteen days respectively. Two animals died under the anaesthetic during the sixth and the tenth administration. The rest were killed by chloroform two days after the last administration, and the liver and kidneys immediately removed. In each case the anaesthesia was induced under a bell-jar, and maintained by hand with chloroform sprinked on a small piece of lint. Of the two animals given ether, one was given six, and the other fourteen, half-hour administrations as above. The induction was made under a bell-jar, and anaesthesia was maintained by ether on an open mask.

The liver and kidneys of all these animals appeared normal on removal.

Microscopically.—The liver of all the animals which had chloroform exhibited definite fatty degeneration scattered over the lobules, but particularly in the central part; but two specimens only showed necrosis—that is, the specimens from the animal which had one hour's continuous anaesthesia, and that from the animal which had ten half-hours. The necrosis was very slight, and confined to a few cells round the central veins of the lobules.

In the kidneys there was fatty degeneration, more or less marked, in all the specimens except one, curiously enough that being from the animals which had had the greatest number of administrations.

The changes were chiefly noticeable in the loops of Henle and around the collecting tubules. In one specimen it was very marked in the convoluted tubules.

In the specimens from the animals given ether inhalations the liver was congested and the capillaries very dilated towards the centre of the lobules, the cells appearing somewhat granular in this position. There was not fat or necrosis.

The kidneys appeared practically normal.

TABLE II.—*Inhalations of Chloroform and Ether.*

No.	Dose.	Date of Death.	Liver.		Kidney.
			Necrosis.	Fatty Degeneration.	Fatty Degeneration.
1	Half an hour chloroform	Died 1st day	−	+	+
2	1 hour chloroform	Killed 2nd day	+	+	+
3	6 half-hours chloroform	Died under chloroform	−	+	+
4	10 half-hours chloroform	Died under chloroform	+	+	+
5	13 half-hours chloroform	Killed 1st day	−	+	+
6	14 half-hours chloroform	Killed 1st day	−	+	−
7	6 half-hours ether	Killed 1st day	−	−	−
8	14 half-hours ether	Killed 1st day	−	−	−

In the third series of experiments (Table III, p. 626) 13 animals were infected intraperitoneally: 1 with a pure culture of streptococci; 4 with a pure culture of *B. coli*; 6 with pus taken from an appendix abscess (containing streptococci and *B. coli*); 2 with pus from an abscess (containing streptococci and staphylococci).

In addition, four of the above rabbits were given ½ c.cm. of chloroform subcutaneously on the day subsequent to infection. Some difficulty was experienced in producing an extensive infection of the peritoneal cavity by injection of emulsions of pure cultures of streptococci and *Bacillus coli*, but pus taken direct from an abscess was eventually injected and proved the better method. All the above animals developed general peritonitis, with the exception of the last two of the series—that is, those injected with a mixed infection of streptococci and staphylococci. In both of these animals there was a large localized abscess.

In all the animals, save one, which had had chloroform as well as being infected, fatty and necrotic degenerative changes occurred in the liver and kidneys, the microscopical appearance being similar to that in the first series (Table I). The dose of chloroform given in each case was a non-lethal one, and the degenerative changes were more severe than one would have expected had the animal had chloroform alone. In those animals which had peritoneal infection alone, the microscopical changes were as follows:

In the liver there was marked dilatation of the capillaries towards the centre of the lobules and the cells appeared swollen, and in three specimens there were definite fatty degenerative changes. In addition, however, in the frozen sections the cells in the central part of the lobule appeared as if loaded with granules which stained rather darkly. In the paraffin sections a large proportion of the granules had disappeared and those cells were minutely vacuolated, giving the centre of the lobule a broken appearance.

TABLE III.—*Infection and Infection + Chloroform.*

No.	Injected with.	Date of Death.	P.M. Findings.	Effects.	
				Liver.	Kidney.
1	Streptococci	Died 3rd day	General peritonitis	Fatty degeneration	Fatty degeneration.
*2	*B. coli*	Died 2nd day	General peritonitis	Granular change	Cloudy swelling.
3	*B. coli*	Died 2nd day	General peritonitis	Fatty degeneration	Fatty degeneration.
*4	*B. coli*	Killed 2nd day	Peritonitis and abscess	Granular change	Granular change of epithelial cells.
*5	*B. coli* + ½ c.cm. $CHCl_3$	Died 2nd day	General peritonitis	Granular change	Cloudy swelling.
*6	Pus from app.	Killed 5th day	General peritonitis	Granular change	Cloudy swelling.
*7	Pus from app.	Died 1st day	General peritonitis	Granular change; some fat	Cloudy swelling.
*8	Pus from app.	Died 4th day	General peritonitis	Granular change	Cloudy swelling.
*9	Pus from app.	Died 3rd day	General peritonitis	Granular change	Cloudy swelling.
10	Pus from app. + ½ c.cm. $CHCl_3$	Died 2nd day	General peritonitis	Fatty degeneration; necrosis	Fatty degeneration.
11	Pus from app. + ½ c.cm. $CHCl_3$	Died 2nd day	General peritonitis	Fatty degeneration; necrosis	Fatty degeneration.
12	†Pus + ½ c.cm. $CHCl_3$	Killed 3rd day	Abdominal abscess	Fatty degeneration; necrosis	Fatty degeneration.
*13	†Pus	Killed 3rd day	Abdominal abscess	Granular change	Cloudy swelling.

*In these specimens the cells of the inner portion of the liver lobule in the frozen sections appeared to be crowded with granules. In the paraffin sections there were extensive minute vacuolations in the place of the granules, the cells having a broken appearance. The renal epithelium showed the same phenomenon to a less marked extent.
†Streptococci and staphylococci.

It would appear that this granular condition represents a transitional stage in the formation of fat in the cell, as comparisons with specimens where both fatty and granular changes occur reveals the fact that this change is best seen in those cells in close proximity to the advancing line of fatty degeneration.

In the kidney there was well-marked cloudy swelling in nearly every specimen and granular changes in the epithelial cells, similar to that described in the liver cells, in two or three specimens. Definite fatty degeneration was seen in the loops of Henle in two cases.

The foregoing series of experiments, although admittedly fragmentary and incomplete, serve to bring out some points which may be of help to the anaesthetist, particularly as regards choice of anaesthetics.

They show us that chloroform has not only a profound effect on the cells of the liver—a fact which has frequently been shown—but that it has an almost equally harmful effect on the cells lining the tubules of the kidney—a fact which has never been sufficiently emphasized. The damage done to the kidney seems to me of perhaps even greater import than that done to the liver. For the chloroform not only acts on the liver cells and interferes with the metabolism of fats, thereby throwing out into the blood the poisonous fatty acids which cause an acid intoxication, but by its injury to the kidney cells interferes very seriously with their chief means of excretion. They show also that ether, even in very large doses, does not have this harmful action on the cells of the liver and kidney to anything like the same extent.

The fact that the damage done in those animals which had subcutaneous injections was much greater than in those which had inhalations would seem to indicate that the higher the percentage of chloroform in the blood the greater its harmful effects, so that a few minutes' very deep anaesthesia may do more damage than an hour's light anaesthesia, emphasizing the importance to the anaesthetist of giving the minimum dose of chloroform to his patient consistent with the requirements of the surgeon.

The inhalation experiments are of particular interest as showing the extraordinary amount of damage done by a comparatively short chloroform anaesthesia, one hour's anaesthesia producing extensive necrotic changes in the liver lobules, in addition to well-marked fatty degeneration in liver and kidneys. It will also be seen that fatty degeneration was present in the liver of every animal given chloroform in this series, and in the kidney in every case save one. No change whatever occurred in either of the animals given ether. It is not suggested for one moment that chloroform should be discarded as an anaesthetic, as some American writers would seem to suggest, but it is of the utmost importance that those of us who use it constantly should fully realize the harm that this powerful drug may do, and administer it in minimum doses.

In those animals whose peritoneal cavities were infected considerable difficulty was experienced in producing a peritonitis by injection of emulsions of cultures of streptococci and *B. coli*, and in only the few given in Table III was there any success, and it was not until injections of pus taken direct from an abscess were injected that there was any certainty of a result. It will be seen that in only two of the animals did a definite fatty degeneration occur in the liver and kidneys, but in all the others definite degenerative changes did occur, as described above, the liver cells appearing to be loaded with small granules in the frozen sections, but in the paraffin sections the same cells were vacuolated and had a broken appearance in the central part of the lobule. Four animals in the above series were of particular interest—that is, Nos. 8, 9, 10 and 11 (Table III). All were given an injection of pus from the same appendix abscess (containing streptococci and *B. coli*) on the same day. On the day following the two animals which appeared the least ill were given a non-lethal dose of ½ c.cm. of chloroform. These two animals died on the second day after the injection of pus, while the two which did not have the chloroform lived to the end of the third and the fourth day respectively.

Having obtained the fact, therefore, that acute infections, at any rate of the peritoneum, do cause definite degenerative changes in the liver, and changes, though less marked, in the kidney, it seems of importance that in cases of this kind chloroform should not be given unless absolutely necessary, on account of its harmful effects on the same organs, and that ether, which has not the same harmful action, should be used whenever possible.

Glucose has now for some time been given to counteract the evil after-effects of chloroform, and I have long since proved its value in my own practice, but the series of experiments in Table IV will prove of interest (see p. 627).

Ten rabbits were all given 1 c.cm. of chloroform subcutaneously, five of them having been fed with glucose the day previous to injection, and some was mixed with their food on subsequent days. Of those which had no glucose, three died, and the other two were extremely ill, while none of those given glucose died, nor did they appear ill at any time.

It is curious that very little difference was found in the amount of damage done in the liver and kidney cells between those which had had glucose and those which had not, except that in the former the areas of fatty degeneration were mapped out definitely, while in the latter the areas of degeneration blended gradually with the healthy cells around.

TABLE IV.—*Chloroform and Chloroform + Glucose.*

No.	Dose.	Date of Death.	Fatty Degeneration.	
			Liver.	Kidney.
1	1 c.cm. chloroform + glucose	Killed 5th day	+	Slight
2	1 c.cm. chloroform + glucose	Killed 3rd day	+	Slight
3	1 c.cm. chloroform + glucose	Killed 3rd day	+	+
4	1 c.cm. chloroform + glucose	Killed 4th day	+	+
5	1 c.cm. chloroform + glucose	Killed 4th day	+	+
6	1 c.cm. chloroform alone	Killed 5th day	+	+
7	1 c.cm. chloroform alone	Died 2nd day	+	+
8	1 c.cm. chloroform alone	Died 2nd day	+	+
9	1 c.cm. chloroform alone	Died 3rd day	+	+
10	1 c.cm. chloroform alone	Killed 4th day	+	+

Finally, I have to thank Mr. Foulerton, of the Middlesex Hospital Clinical Laboratory, for kindly injecting some of the animals for me, and Mr. Hillier for much help and advice; also Dr. Lakin and Dr. Strickland Goodall for checking my interpretation of some specimens, and Mr. Annison for doing many sections.

DISCUSSION.

Dr. LEVY (London) said that the inhalation experiments of Mr. Apperly had the greater clinical significance. He was rather at a loss to understand the cause of death in some of these inhalation cases; for instance, No. 1 in Table II, as he was not aware of any parallel observations in other animals—for example, cats and dogs.

Dr. A. EHRENFRIED (Boston, U.S.A.) said that some research on similar lines had been done in America. Acidosis was, however, rare in that country owing to the more general use of ether. He thought there was always a greater risk of its occurrence when a second administration of an anaesthetic followed within a few days, and for this reason always counselled the allowance of a considerable interval between two operations on the same patient.

Dr. MCCARDIE (Birmingham) was glad to hear that Mr. Apperly's statement that he had found no difference in the action of chloroform prepared from methyl alcohol, methylated spirit, and acetone agreed with general clinical experience. There seemed to be four varieties of chloroform poisoning: (1) Ordinary delayed chloroform poisoning, especially in the young; (2) acute yellow atrophy of the liver, especially during parturition; (3) the haemorrhagic variety, with obstruction of blood in the liver and haematuria and longer course; (4) cases in which there was urinary suppression. He asked if Mr. Apperly had seen in his researches on animals evidence of the existence of any of these types. He himself feared the remote action of chloroform as much or even more than its immediate danger.

The PRESIDENT pointed out that a number of factors, clinical and experimental, interfered in drawing definite conclusions. Some animals were particularly liable to the fatty changes described, so he thought that such experiments should be conducted with other animals as well as rabbits; cats and dogs would, perhaps, have been better. Rabbits were particularly liable to asphyxiation during anaesthesia. He thought caution was necessary before adopting these results in clinical work. He asked if the rabbits were wild, how they had been fed, and had any change been made in their life routine shortly antecedent to the experiments. A good deal of experimental work had been done with regard to this subject, and recently some valuable papers had appeared in the *Johns Hopkins Bulletin* and elsewhere. In all these researches, as well, so he gathered, as in that of Mr. Apperly, no sufficient care was taken to avoid high percentage vapours being inhaled. It was too often forgotten that it was the concentration of the vapour which counted, even more than the actual amount entering the tissues. It was definitely proved by Strassburg and the earlier workers that chloroform toxaemia would in some persons produce fatty changes; it was yet to be shown whether chloroform narcosis conducted by dosimetry ever produced this result. The premature deaths of rabbits numbered 3, 4, 7, 8, and especially 9 and 17, seemed to the speaker to indicate that these animals had been subjected to vapours of toxic concentration, unless other determining causes of death were at work.

Mr. APPERLY, in reply, said that acetone always appeared in the urine after the administration of anaesthetics, and being more rapidly secreted after ether appeared in greater abundance in samples of the urine. In his experiments he had not noticed the four clinical types mentioned by Dr. McCardie. His rabbits were wild rabbits which had been in captivity for a long period, and no recent changes in diet or routine of life had been made previous to the experiments. He had killed and examined the organs of several similar rabbits as controls, but had never found the changes in them which were present in those the subjects of the experiments.

A CARDIAC EFFECT OF ADRENALIN IN CHLOROFORMED SUBJECTS.

By A. GOODMAN LEVY, M.D., M.R.C.P.,
Physician to Out-Patients, City of London Hospital for Diseases of the Chest.

In the course of some investigations upon the heart beat in animals under chloroform I have found that adrenalin exerts a very pronounced cardiac effect, and one that has an important clinical significance.

Adrenalin belongs to a group of substances which are styled by pharmacologists "sympathomimetic," of which the action is limited to structures which are innervated by fibres of the sympathetic system. This action is originated in the terminations of the sympathetic distribution, and it results in a rise of blood pressure, mainly caused by vaso-constriction in those vessels in which the constrictor fibres predominate, but also reinforced through stimulation of the augmentor nerves of the heart, which is thereby accelerated and caused to beat with increased force. This direct cardiac action is apt to be overlooked, for it is obscured by the more obvious vaso-constrictor effect, but there is good reason for attributing to it and to it alone the abnormal condition which arises when adrenalin is injected into the blood of an animal under chloroform.

If a small dose of adrenalin chloride, say, half a minim of the commercial 1 in 1,000 solution, be injected into a vein of a cat fully under the influence of chloroform, a well-marked rise of blood pressure will be indicated by a manometer connected with the circulation. If the dose of adrenalin be sufficiently small and the anaesthesia be sufficiently deep, then the heart may be found to continue to beat perfectly regularly; more frequently, however, as the adrenalin effect develops, the heart passes into an irregular condition of a remarkable type. The obvious features of this condition are a rapid succession of small beats, about 300 per minute, the individual beats being sometimes almost equal in size and in time incidence, sometimes more irregular and interrupted by pauses of short duration. Electrocardiographic tracings demonstrate that this irregularity is one of a singularly complex type—in fact, it frequently happens that not a single normal beat can be found in the sequence, which consists entirely of ventricular extrasystoles; that is to say, the contraction impulses arise in the ventricles themselves and not in sequence to auricular contractions. Moreover, the ventricular contractions are propagated, not from one but from many points of origin in both the right and left ventricles, constituting a condition which may be defined technically as a multiple extrasystolic ventricular tachycardia. After the effect of the adrenalin, which, as is well known, is only temporary, has worn off, the heart quiets down and the beat eventually becomes regular once more.

If the same injection be made into an animal only lightly anaesthetized with chloroform the resulting reaction is ultimately more intense; it is, in fact, almost

invariably fatal. To ensure the success of such an experiment the vapour must be reduced below 1 per cent., the corneal reflex being retained and active. We will presume the heart, under such conditions, to be beating with a normal, regular rhythm when half a minim of solution of adrenalin chloride is injected into a vein; the heart passes, as before, into an intensely irregular condition of the type previously described. The final result is, however, different, for before the action of the adrenalin has worn off, and whilst the blood pressure is still abnormally high, the heart, with a startling suddenness, ceases to propel blood into the arterial system, and in a graphic record the blood pressure is seen to sink to zero in the precipitate fashion shown in the tracing (Fig. 1). This is an almost invariable effect. What has happened is this: Up to the moment of the collapse of the circulation the ventricles have been contracting in a chaotic sequence of irregular beats, but each beat is still a *co-ordinate* beat; each ventricle contracts as a whole and expels its contents. The irregularity then advances a step further; the individual muscle bundles of the ventricles, which interlace in many directions, commence to contract *independently* and in sequence, the beat is no longer co-ordinate, and as a consequence the ventricles cease to exercise an active function. The ventricles are now said to be "fibrillating"; the electrocardiogram shows a sudden and characteristic change in form; the contraction impulses are still generated, but they are totally disorganized.

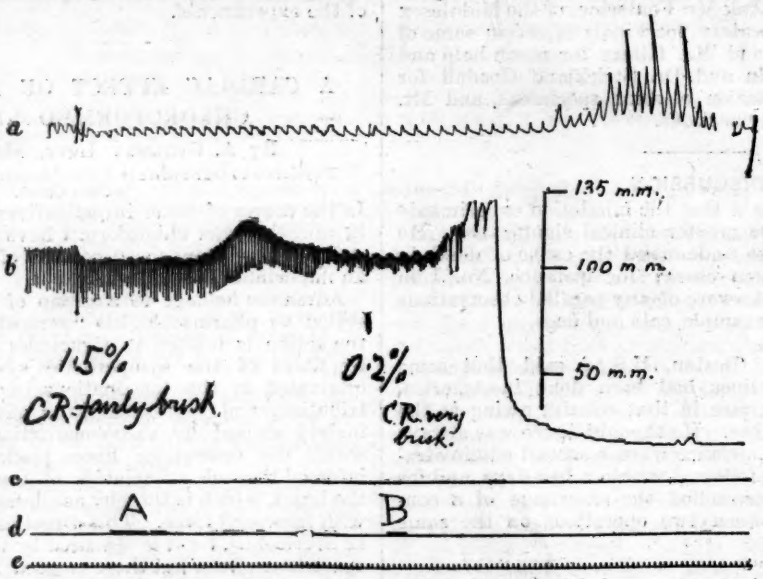


Fig. 1.—*a*, Respiration curve, registered from thoracic movements. *b*, Blood pressure curve, registered by mercury manometer. *c*, Level of zero pressure. *d*, Signal line; each signal mark indicates an intravenous injection of ½ minim of 1 in 1,000 adrenalin solution diluted to 1 in 20,000. *e*, Time marker, indicating intervals of one second. The vertical scale indicates blood pressure levels. The tracing represents the effect of two consecutive injections of adrenalin (A and B) into the same cat under different depths of chloroform narcosis; A, under 1.5 per cent. chloroform; B, under 0.2 per cent. chloroform. The result of A is a rise of pressure only, the heart beat remaining regular. The result of B is likewise a rise of pressure, but with it the heart beat becomes small, rapid (300 per minute), and irregular, and finally ceases abruptly. This cardiac collapse occurs some eighteen seconds after the termination of the injection, and the respiratory movements, after passing through an exaggerated phase, cease also some thirty-eight seconds later.

The physiological effect of ventricular fibrillation is very different from that of auricular fibrillation—the heart can continue to function in the absence of the auricular systole—in the absence of the ventricular systole the circulation naturally comes to a standstill. Ventricular fibrillation in cats is generally permanent and ends in death of the heart from asphyxia of its tissues, but exceptionally spontaneous recovery occurs, and this may be temporary, culminating in a second collapse (Fig. 2, p. 629), or it may terminate more favourably in a regular beat and permanent recovery. The prospect of recovery is greater when the doses of adrenalin are very small: the smallest dose I have so far found efficient in a cat has been one-fifth of a minim, and in this case recovery ensued.

The respiratory phenomena in connexion with this form of heart failure are noteworthy—the bulbar centres, which are not depressed as from a full dose of chloroform, are suddenly emptied of blood, and the anaemia stimulates the respiratory centre powerfully; a number of deep gasps are taken and then the respiration ceases (Fig. 1). The respiratory centre, however, is not yet *dead*—it has not been killed by excess of chloroform—spontaneous respirations may arise later, and can be almost invariably evoked by artificial respiration, and the late persistence of respiratory effort in the absence of heart action is a marked feature in many of these deaths.

In animals not under the influence of any anaesthetic adrenalin does not, in my experience, produce irregularities of the nature I have just described, and under ether I have employed very much larger doses of adrenalin than those which react under chloroform, up to 5 minims of the 1 in 1,000 solution, in fact, with the result that lesser degrees of irregularities were exceptionally observed, and never fibrillation.

In concluding this brief account of experimental work I may remark that I have here merely skirted the fringe of the subject of cardiac irregularities under chloroform, and I must refer those who desire further information to such notes as have already been published on this subject.[1] I have, however, been prompted to anticipate a more complete account by reason of evidence that has recently come to light that this particular reaction of adrenalin under chloroform has an important clinical bearing, as I shall now proceed to show.

The symptomatic effects of an adrenalin reaction in a person lightly under the influence of chloroform may be portrayed as follows: Within a few seconds of the injection the pulse accelerates with a raised tension; it becomes more rapid, is less readily felt owing to diminished excursion, and it may or may not be perceptibly irregular at the wrist; a few temporary pauses may be noted, and then the heart suddenly ceases beating; the pupils dilate widely, and intense pallor supervenes. Respiratory phenomena then follow, a few deep breaths being taken, and then the respirations cease also. A few spasmodic efforts at respiration may be made, or even a short term of regular breathing may be induced by artificial respiration, but the heart still remains impotent. The heart may, however, recover temporarily, still with a small and rapid beat, only to fail once more, and in this way the pulse may be briefly felt *after* the respiratory collapse has occurred. If the cardiac recovery is well sustained the respiration is restored also, only to fail again with the second collapse of the heart. In more favourable cases the heart eventually becomes regular, the breathing is restored permanently, and all goes well.

My attention was first directed to the chloroform-adrenalin reaction in man by a communication made by Dr. Blumfeld, and published in the *Proceedings of the Royal Society of Medicine*, February 3rd, 1911, Anaesthetic Section.

In this case an injection of adrenalin was made into the mucous membrane of the nasal septum, the proposed operation being a submucous resection of a deviated septum. The amount injected was 6 minims of a 1 in 4,000 solution, which is the equivalent of 1½ minims of the commercial 1 in 1,000 solution. The anaesthetic was a chloroform and ether mixture; no anaesthetic was being administered at the moment of injection, and the patient's anaesthesia was light. Sudden pallor and respiratory failure rapidly followed the injection—the pupils dilated, and for a minute or so the patient's condition was serious, but he recovered completely. The condition of the pulse was not noted.

This experience was confirmed by others who took part in the discussion on Dr. Blumfeld's paper, and, further, two fatalities under similar circumstances were referred to. In one of these cases, briefly reported, death followed the injection of a little adrenalin into the tonsils of a patient

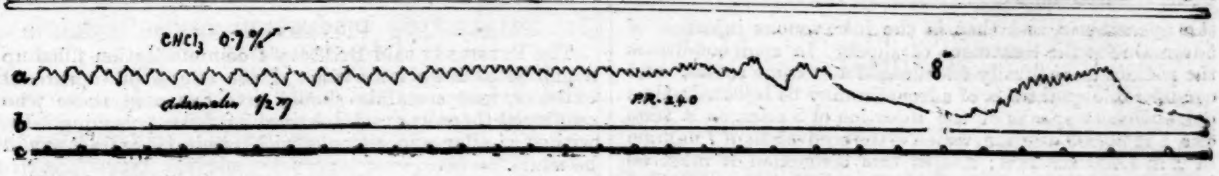


Fig. 2.—*a*, Blood pressure curve registered by Hürtle manometer. *b*, Signal line and level of zero pressure. *c*, Time marker indicating seconds. The gap in the tracing represents an interval of eight seconds. The Hürtle manometer affords a more correct interpretation of the heart beats than the mercury manometer. The drum is moving more rapidly than in Fig. 1, and the character of the individual beats is thus made more distinct. The tracing represents the effect of the injection of ½ minim of 1 in 1,000 adrenalin solution diluted to 1 in 20,000. At the time of injection the heart was beating in pairs (bigeminal beat). The change in the character of the heart beat is well shown. Ventricular fibrillation with total cessation of the heart's action occurs about fourteen seconds after the injection, but in this case temporary recovery occurs eleven seconds later, the gap in the tracing representing a period of eight seconds, during which the heart is inactive. The heart is still irregular on recovery, and fibrillation recurs in four seconds, from which the heart does not this time recover.

under chloroform, but of this case I have not been able to obtain further information. The second case was mentioned by Dr. A. L. Flemming, who has kindly sent me further particulars, and these I will give in a later part of this paper.

In the BRITISH MEDICAL JOURNAL of February 24th, 1912, there appeared an article by Mr. B. Seymour Jones on anaesthesia for submucous resection of the septum. Mr. Jones has a record of some 100 cases in which adrenalin was injected (in conjunction with cocaine) into several sites on the septum, for the usual purpose of rendering the seat of operation bloodless, the total amount of adrenalin on completion of all the injections being 12 minims. The anaesthetic was a chloroform-ether mixture, and the injections were made when the patient was "barely unconscious." I gather that a not infrequent result of the injection was an "intermittent beating associated with a pulse of a weak and feeble character," and three cases are particularly quoted to illustrate the alarming effects of a sudden absorption of adrenalin. In each of these cases the symptoms noted were intense pallor, cessation of respiration, and dilated pupils; in one case the pulse was felt for, but was not found; in all permanent recovery ensued. In one of these instances the injection consisted of a few drops of adrenalin in ½ c.cm. of normal saline without cocaine. No serious effects were noted in a series of 500 cases in which similar injections were made for operation without any general anaesthetic.

There can be no doubt that in all the foregoing cases the ventricles fibrillated; doubtless the large proportion of recoveries recorded is largely due to a small quantity only of adrenalin passing into the circulation, but their number is remarkable when we take into consideration the deadly nature of the condition of ventricular fibrillation. At one time it was an open physiological question whether fibrillating ventricles ever recovered their normal beat, but the chloroform experiments above referred to definitely settle this question in the affirmative so far as the cat is concerned, although recovery is exceptional. It is evident from these clinical cases that the human heart has a greater capacity for recovery than has the feline heart, but at the same time I have no doubt that this capacity is made to appear greater than it really is through failure to report fatal cases. Apart from the two fatal cases referred to above, I know of none published. Private inquiry has, however, proved more fruitful. Thus:

Surgeon No. 1 had been accustomed to plug the nostrils with adrenalin twenty minutes *prior* to the administration of chloroform for submucous resection of the septum; he had met with no circulatory difficulties whatever.

Surgeon No. 2 had been accustomed to *inject* adrenalin for the same operation when the patient was lightly under chloroform; and with him my inquiries ceased, as it was unnecessary to pursue them further. This gentleman had "two cases of rapid death after the injection of adrenalin into the nasal septum in cases under chloroform. Both were puny adolescents, one male and one female. In the male a positive diagnosis of status lymphaticus was made; in the female I believe this was not the case." Both cases were lightly anaesthetized, and in both the symptoms were similar. These were as follows: The ischaemia of the septum appeared to spread to the lips and circumoral region, the heart beat violently and rapidly, the pupils became widely dilated, and deep sighing respirations were followed by cessation of breathing. The time relations of these symptoms were apparently not accurately noted, except in so far as that they certainly almost immediately followed the injection. The quantity of adrenalin injected was "a very few drops" of 1 in 4,000 solution with 2 to 3 per cent. novocain. In reply to further questions, he proceeds:

"The peculiar thing is that the very great majority have not shown the slightest disturbance on injection; are some cases especially susceptible? I have been inclined to believe that in the dangerous cases the injection has got directly into the circulation through a small vessel. There is a network of veins at the lower anterior part of the septum in the *submucous* tissue, but one endeavours to inject under the perichondrium, where there are no vessels."

These two cases* afford absolute confirmation of the occurrence of the adrenalin death in human subjects, and that with doses which are extraordinarily small compared with the lethal doses in the cat, having due regard to the relative blood volumes. That the reaction only occurs exceptionally in a series of cases, all treated alike, may be subject to one of two explanations: (*a*) Presuming adrenalin to be readily absorbed in all cases from submucous tissues, then the reaction may be said to occur in persons abnormally sensitive to small doses; (*b*) presuming all persons to be equally sensitive to small doses, then the exceptional results may be referred to accidental direct injection into a small vein.

It is difficult to decide definitely on either of these alternatives, and I can only here make a few brief remarks appertaining to this subject. It is well known that on subcutaneous injection adrenalin is, in the generality of instances, only very slowly absorbed, owing to the local vaso-constriction produced; it appears, however, to be certain, from the symptoms entailed, that adrenalin is rapidly absorbed when injected into the gums for dental purposes,[a] and it is asserted that vaso-dilator fibres predominate in the gums, at least that they do so in the dog, and hence the adrenalin would have a better chance of absorption. I cannot discuss this further, but will proceed to quote a case communicated to me by Dr. de Prendeville, which appears to support the absorption theory. The patient was under C.E. mixture, and, contrary to usual custom, the nose had not been packed with cocaine and adrenalin. The surgeon suddenly remembered this, and sprayed some 1 in 1,000 adrenalin solution into either nostril. Within fifteen seconds of this a typical train of symptoms occurred, and the patient was pulseless for two minutes, but she eventually recovered. In this case there can be no question, I presume, of puncture of tissues, and the result is probably due to surface absorption alone.

Apart from this question of absorption, it stands to reason that the reaction is favoured when the needle punctures a vein or venous plexus and the drug passes directly into the circulation, and we must be content to leave the problems of absorption and susceptibility to a certain extent open questions for the present.

The operation of resection of the septum and similar ones on the naso-pharynx are peculiarly liable to exhibit bad results from adrenalin injections owing to well-defined reasons: (1) Chloroform or a mixture containing chloroform is employed in order to obviate unnecessary congestion; (2) a light anaesthesia is insisted upon, so that the coughing and swallowing reflexes may be maintained, and frequently to enable the operation to be performed in the upright position. The anaesthesia further tends to become light owing to interruption of the administration during the process of injection; (3) the vascular condition of the regions in question is favourable to absorption. There is, however, another procedure involving the use of adrenalin from which I should anticipate a heavy death roll when chloroform has been

* A precisely similar fatality is reported in the *Leicester Mercury* of August 19th, 1912.

the anaesthetic, and that is the intravenous injection of adrenalin for the treatment of shock. In such conditions the patient is naturally encouraged to "come round," and considerable quantities of adrenalin may be injected; thus one authority speaks of the injection of 5 c.cm. at a time of a 1 in 20,000 dilution, which is the equivalent of 7 minims of 1 in 1,000 solution; and in this connexion it must be borne in mind that it is the mass of the drug injected that mainly counts, and not the dilution in which it is introduced, for the principal dilution occurs in the blood itself. Under certain circumstances certainly, the susceptibility to ventricular fibrillation is markedly reduced—for example, after a prolonged administration of chloroform, or after some severe form of operation—but the fact remains that published fatalities are far to seek. Possibly the reaction may not be noted in a moribund patient, the adrenalin effect being confused with shock effects, and very probably such fatalities as occur are never published even when recognized. I am, however, able to present to you the details of one case of death in this category. Dr. Flemming writes as follows:

> This case was one of appendix abscess, left too long, with consequent toxaemia. The anaesthetic was chiefly open ether, but chloroform was also used for the induction. After the operation, which was a hurried one, it was obvious that the patient was going to die within an hour or two, but death occurred suddenly within two or three minutes of the injection, made at the surgeon's request, of 1 minim of adrenalin solution (1 in 1,000). The injection was intramuscular, into the deltoid. Before injection the patient was pale, with shallow breathing, 40 per minute, and with a rapid, small, but regular pulse, about 160 per minute. Upon injection there was a change noticed in thirty seconds—the pulse felt fuller, and the individual beats better defined for about half a minute, but after the improvement had been noted for a very brief period there was a sudden deep inspiration, followed, after an interval of some ten seconds, by another, and there occurred, at the same time apparently, sudden cessation of the pulse. All these phenomena occurred so quickly and so took me by surprise that I can only give you my impressions as noted afterwards.

There can be no doubt that the needle in this case penetrated a vein; and here, again, the dose (1 minim) is remarkable for its smallness. It is notable that chloroform was used only for the induction, but the operation being a hurried one, it had not been eliminated, and remained in a low concentration in the body when the injection was made.

I conclude that the adrenalin reaction occurs in human beings as readily, and perhaps more readily, than it does in cats, and that if commensurate doses were given, it would occur at least as frequently.

I have already mentioned that experimental work excludes ether from participation in the adrenalin reaction, but it is a matter of conjecture that the reaction may be modified in the case of C.E. mixtures, for it is conceivable that with the combined anaesthetics adrenalin may react during a deeper degree of narcosis than with chloroform alone, for the chloroform effect upon the heart would be dependent upon the partial tension of the chloroform vapour only, and the combined anaesthetic effect would be out of proportion to this. It is further probable that under such circumstances the respiratory centre, being more depressed, would react to sudden anaemia in a less intense degree.

The conclusion to be drawn is that it is unsafe to inject adrenalin into the veins or vascular tissues of a patient lightly under the influence of chloroform. It may be safely injected just previous to induction with chloroform—that is, with the patient under no anaesthetic at the moment, or in a patient fully under chloroform, or into a patient anaesthetized to any degree with ether. Whatever the dose and whatever the dilution, and, in fact, whatever the other attendant conditions may be, in the case of light chloroform anaesthesia a risk is always taken which is unjustifiable.

Finally, I may add that the clinical cases herein quoted possess a pathological interest apart from their relation to anaesthetics; they afford the first substantial evidence of the association of sudden heart failure with ventricular fibrillation in the human subject.

REFERENCES.

[1] Levy, *Proc. Physiol. Soc.*, January 21st, 1911, and October 21st, 1911; and May 18th, 1912; Levy and Lowis, *Heart*, vol. iii, No. 1. [2] *Proc. Roy. Soc. of Med.*, Therap. Sec., 1911, p. 59.

DISCUSSION.

The PRESIDENT said Dr. Levy's communication filled up a gap in existing knowledge of the effects of suprarenal extracts, and certainly should give pause to those who employed them as antidotes to chloroform poisoning. He wished similar experiments with pituitary extract could be made.

Dr. MCCARDIE (Birmingham) said he had seen collapse occur immediately after the injection of adrenalin in a septal operation when the patient was fairly deeply under a mixture of chloroform and ether. He had never seen any sign of it during ether anaesthesia. Mr. Seymour Jones, who had in his paper noted the occurrence of three cases of collapse, had performed some 800 operations under local and general anaesthesia, and always insisted on the lightest possible anaesthesia during injection and for three to five minutes afterwards, because of the danger of collapse from absorption of adrenalin during even moderate or deep anaesthesia. He did not use cocaine. Dr. McCardie now always induced anaesthesia by a mixture of chloroform and ether, and continued it with chloroform for these operations. Not uncommonly he found that the pulse became quick, irregular, and weak immediately after the injection, and the colour paler. On the other hand, he himself, when he had had injection of adrenalin for a septal operation, had had rather a feeling of stimulation. Mr. Seymour Jones's, and therefore his, experience was contrary to Dr. Levy's experimental results.

Dr. A. EHRENFRIED (Boston, U.S.A.), in response to an invitation from the PRESIDENT to give the experience of American surgeons, said that, as far as his knowledge went, adrenalin was seldom injected into the nasal mucosa during or previous to operations thereon by surgeons in the States.

Dr. LEVY, in reply, expressed surprise that Dr. McCardie's experience in practice was totally opposed to the results of physiological experiment, which definitely showed that it was only under light chloroform anaesthesia that the phenomena he had been speaking of developed. He considered Dr. McCardie had been extremely lucky.

Memoranda:

MEDICAL, SURGICAL, OBSTETRICAL.

INTRAPERITONEAL INJECTIONS OF OXYGEN DURING CERTAIN ABDOMINAL OPERATIONS.

DURING the last four years I have injected oxygen into the peritoneal cavity directly the abdominal wound has been sutured, and I have found it distinctly beneficial in the following conditions:

1. Tuberculous peritonitis.
2. Septic peritonitis.
3. After the removal of large abdominal tumours.
4. In certain cases to prevent the formation of adhesions.

The method I adopt is as follows: Just before closing the peritoneum of the abdominal wound I insert a small trocar and cannula (specially made for me by Allen and Hanburys) through the abdominal wall a short distance from the original wound, and then withdraw the trocar and leave the cannula in the peritoneal cavity. The abdominal wound is then closed in layers. A sterilized rubber tube from a Wolfe's flask is then attached to the cannula. The Wolfe's flask, which contains hot saline, is connected to a cylinder of oxygen, and the oxygen, warmed, is then passed into the peritoneal cavity. The amount passed varies, but should be enough to obliterate the liver dullness. In cases in which I have filled the abdominal cavity quite tense with the gas, it has taken from ten to fourteen days to be absorbed.

I have never seen this method of giving oxygen used in any other hospital, nor have I seen any account of it in literature, and from the success I have had from it in these special cases I think it is worth a more extended trial.

H. J. GODWIN,
Honorary Surgeon, Royal Hampshire County Hospital, Winchester.

BREECH PRESENTATION IN SUCCESSIVE LABOURS.

DR. ILOTT, in his memorandum on breech presentation in three successive labours, states such a sequence was unknown and worthy of record. A case that I attended on May 20th, 1904, is still more worthy of record. On my arrival I found that the presenting part could not be felt, the os being very high, but the membranes were protruding well through the os. The head could be palpated in the epigastrium with the child's back to the left side of the mother, and a diagnosis of first breech presentation was made. The mother then told me that she had had seven children previously, all of whom, with one exception, had been born by the breech, and all were then living. No doctor had been in attendance in any of these until the third stage, the placenta being always retained. On this occasion labour was tedious, lasting twenty-three hours, but the child (a male weighing 8¼ pounds) was vigorous. The placenta was expressed in half an hour. It must surely be very exceptional for a woman to have seven out of eight children born by the breech, and all alive and healthy.

Latrobe, Tasmania. G. W. DAMMAN, M.B., B.S.Melb.

THE following case may be interesting in connexion with Dr. Herbert J. Ilott's note of June 1st, page 1240, *re* "Breech Presentation in Three Successive Labours."

I was sent for by the matron on May 17th, 1912, to attend Mrs. B., a healthy, robust, and well-built woman, in labour of her fifth child. The presentation was both feet, sex female. As I arrived a little late I found the left arm extended over the aftercoming head, and the child livid and asphyxiated. The extended arm was immediately pulled down, and the head delivered by jaw-traction. After slight efforts at artificial respiration and slapping the infant on the back, it began to cry and recover the normal hue of the skin.

I found that the patient had had feet presentation in all her previous labours: The first, ten years ago, feet presentation, sex male; second, eight years ago, feet presentation, sex male; third, seven years ago, feet presentation, sex female; and the fourth was breech presentation, left sacro-anterior, sex male.

Foot or knee presentations, compared with breech presentations, are supposed to be relatively more frequent with premature children and twins, but in this case all the infants were healthy and well developed. The third baby, according to the mother's version, "must have weighed ten pounds."

This interesting sequence, bearing out Dr. Ilott's of breech presentation, is my excuse for publication, as feet presentations are developed out of breech presentations.

Parsi Obstetric Hospital, Navsari, India. P. M. MEHTA.

Reports
ON
MEDICAL AND SURGICAL PRACTICE IN THE HOSPITALS AND ASYLUMS OF THE BRITISH EMPIRE.

THE BOOTLE HOSPITAL.

A METAL PLATE UNDER THE SCALP—AND THE RESULT.

(By ROBERT TURNER, M.D., Ch.M.Aberd., F.R.C.S.Edin., Honorary Surgeon.)

A MAN, whilst working on board a ship, received a glancing blow from a flying piece of metal. This raised a large flap of the scalp, and cut out a piece of bone about the size of the palm of the hand from the parieto-occipital region in a very neat manner indeed. In hospital the wound healed by first intention, but a large surface of brain was now exposed to possible injuries.

Dr. Flood, late Senior House-Surgeon, took a cast of the region, and Messrs. Arnold made a metal plate to overlap the opening in the skull. A very large flap was dissected down, and the plate fixed in position by three screws. Rapid healing took place, and all went well for about eight months, when the screws became quite loose, and the plate dropped, causing pressure necrosis at the lower pole. Removal of the plate was then necessary.

Reviews.

THE SCIENCE OF SANITARY ADMINISTRATION.

DR. SEATON'S book on *Infectious Diseases and their Preventive Treatment*[1] contains the substance of the Chadwick Medical Lectures for 1910, delivered by invitation of the University of London. It deals with the science of sanitary administration. Few men have had such experience in this field as Dr. Seaton, who has been from early life onwards associated with the most prominent originators of the public health service, and who was officially connected with the institution of the notification and hospital isolation systems at the very outset.

The "stamping out" idea with which public authorities started some forty years ago seems generally, he says, to have been forgotten, and our apparent want of complete success in carrying out the main intention of the Notification Act has been due to failure in realizing the importance of purely preventive measures. It is the recognition of early cases and carriers of infection that must constitute the basis of a rational preventive system. The difficulty, of course, is the impossibility of this early recognition in the vast majority of cases, the result being that the epidemic may have got a solid footing in a district before its true nature is known. In the absence of certain means of early diagnosis the application of the machinery for the prevention of an epidemic is uncalled for. On the other hand, when recognized it is often too late. From this dilemma the only way of escape lies in the discovery of a means of detecting the first spark before it has become a conflagration. Dr. Seaton contrasts the dreams of sanitary perfection which used to be indulged in in the early mid-Victorian era with the practical work done at the present day. In respect of individual hygiene, including the care of infants and the physical condition of children, more has been done to advance the medical part of public health work during the last decade than in the twenty years before. There is, we venture to think, another side to the picture. The care for the physical wellbeing of children is not always carried out in a very enlightened spirit. Zeal sometimes outruns discretion, and the result will probably be that any neurotic tendencies already innate in the child must be largely increased by the minute attention to health that is forced upon them and their parents.

Dr. Seaton thinks that if measles is ever made notifiable and cases, especially those likely to be severe, are removed to hospital, the type may reasonably be expected to change for the better, with the result that the death-rate would be reduced much in the same way as has occurred with scarlet fever. One difficulty occurs to us here, and that is the notion ingrained in the public mind that measles is almost a negligible indisposition. People who should know better scoff at anxious mothers for calling in a doctor to a child suffering from measles, and it is well known that parents often take the opportunity of one child contracting the disease to expose all the others there may be to the infection in order "to get it over." Even in these days of the school teaching of hygiene, we fear that people in general still fail to recognize that in measles, however trivial it may seem in a particular case, lie the possibilities of serious mischief during its course and later on. Notification, with the consequent isolation, would undoubtedly tend to prevent subsequent developments, such as tuberculosis.

Dr. Seaton argues strongly for notification, but with the proviso that the authority applying for notification powers should show that it is prepared to make suitable use of the information obtained. He refers to cases within his own knowledge where nothing has resulted save the tabulation of certificates. This pedantry of officialism is writ large over the whole history of sanitary administration. Scarlet fever at present causes far less mortality than measles. The disease is of milder type than it used to be. But its very mildness tends to lessen the usefulness of notification which comes too late. It is to this failure of the

[1] *Infectious Diseases and their Preventive Treatment.* By E. C. Seaton, M.D.Lond., F.R.C.P., F.I.C., Consulting Medical Officer, Surrey County Council, etc. London: University of London Press; Hodder and Stoughton, and Henry Frowde. 1911. (Demy 8vo, pp. 230; 25 diagrams. 7s. 6d. net.)

notification system that the incomplete success of hospital isolation is largely due.

A chapter is devoted to the etiology of diphtheria and the history of preventive measures in England. Dr. Seaton, while admitting that it is beyond dispute that the great advance in knowledge has already led to some wonderful results, points out that there has been no "vanishing" of the diphtheria death-rate under the influence of the bacterial system, and that the twelve years after Budapest compared with the twelve years before do not exhibit any such marked change as had been predicted in 1894. What took place was a substantial fall of 17 per cent. in the diphtheria mortality of England and Wales. That diminution cannot, according to Dr. Seaton, be ascribed wholly to the bacterial system. Better sanitation and increased attention to diseases of the cow, with consequent improvement of the milk supply, have produced some effect. Increased attention to school hygiene must also be credited with a fair share in the reduction of the death-rate. Here, as in measles, the difficulty is to detect the beginning of an epidemic.

Typhoid fever is another disease which shows a marked mitigation in severity. The fact that it still exists is ascribed by some to mistaken views as to its etiology and epidemiology. In 1873 Gull said that the waterborne doctrine was a very good working hypothesis, but nothing more. To the rising school of epidemiologists, whose researches had done much to throw light on the causation of typhoid, Gull's statement appeared both inappreciative and misleading. Dr. Seaton says, however, that looking back from our present standpoint it may be well asked whether Gull's assertion was very far wrong. By 1893 it had become manifest that a polluted water supply is not by any means the sole or the general mode of conveyance of the disease, and attention was then directed to such agencies as dust and flies, and, above all, to polluted or unwholesome food. The fifteen years, 1893 to 1907, supply negative evidence even stronger in character. By this time notification had become universal, and medical officers had become very active. Dr. Seaton's own observations in the county of Surrey are summarized in the statement that when over 2,000 cases occurring in twelve years had been carefully investigated, it was apparent that only a very small percentage were attributable to the water supply. He is strongly of opinion that hospital isolation in cases of typhoid should be promoted where persons are attacked in crowded or insanitary houses. It is in such circumstances that direct infection frequently takes place.

Dealing with epidemic small-pox, Dr. Seaton says that ever since the adoption of the Notification Act the principle upon which many scientific authorities act in face of a rising epidemic has been to secure revaccination when and where it is most wanted—a very different matter from the aim to secure a total immune population as in Germany or Japan. "The most essential means of prevention is, of course, vaccination *properly applied*." In this country the long-continued freedom from epidemic small-pox is primarily due to vaccination. Without vaccination and revaccination we could not have enjoyed this immunity, for the simple reason that no small-pox isolation hospital could have been staffed. But auxiliary measures of protection are also necessary. An important step in limiting outbreaks is the establishment of a cordon of protected persons around the infected centre. If a case is taken to hospital the attendant and nurses must all be efficiently vaccinated or revaccinated. Without this precaution no small-pox hospital could be worked. Where the cases cannot be taken to hospital all persons likely to come in contact with them should also be vaccinated or revaccinated. Speaking of Leicester and its system of isolation by a cordon protected by vaccination, he says this is the opposite of the unlimited use of vaccination such as is aimed at in Germany. In the one case vaccinations have to be reckoned by the hundred thousand and in the other by the hundred. Here Dr. Seaton takes occasion to call attention to a fallacy which is likely to mislead many people by the belief that the Leicester experiment is a proof that vaccination is unnecessary. The fallacy consists in the confusion of two totally distinct things, namely, the general and the particular use of vaccination in the preventive treatment of epidemic small-pox. Although it is true that Leicester has been able to dispense with universal vaccination, it is equally true that through its officials it has been in large measure protected all the time by vaccination specially applied.

Phthisis is discussed in a chapter in which is recorded the growth of scientific opinion on the influence of the sanatorium. From a review of the question of prevention, regarded from the administrative point of view, he concludes that if county councils are to have an actual or implied responsibility in providing sanatorium treatment, many fresh arrangements will become necessary. Among the chief points for consideration will be the employment of existing hospitals for the reception of advanced as well as early cases of phthisis.

A chapter on public health organization brings this interesting book to a close. It is very suggestive reading, is very richly documented, and is illustrated with charts and other paraphernalia that delight the heart of Mr. Gradgrind, while at the same time it is written in a spirit of judicial fairness and scientific caution that must commend it to men of all shades of thought in regard to public health. It is a storehouse of facts, and represents the conclusions arrived at by a man of the ripest experience in public health administration. We hope it will be widely read, not only by members of the medical profession but by the public, and especially by all who have anything to do with the important function of guarding the health of the community.

CONSUMPTION IN GENERAL PRACTICE.

THE need for care and precision in the diagnosis of tuberculous affections and for well-balanced judgement in dealing with them has never been greater than at the present time. The literature on the subject is becoming voluminous, and the remarkable unanimity amongst writers in all countries is as welcome as it is unusual. Early diagnosis is universally recognized as all-important, and it is to the general practitioners of the world that we must look to take the initiative in discovering the presence of the disease before it has had time to do much mischief. Hence such a work as has lately achieved the distinction of a second edition, written by an experienced sanatorium superintendent and putting the case from the general practitioner's point of view, is an acceptable addition to the bibliography of the subject.

Dr. HYSLOP THOMSON, in the book to which he has given the title *Consumption in General Practice*,[2] sets forth the whole case in simple terms, admirably arranged, and although there is probably but little new to record, still the opinions of a writer of wide experience deserve consideration and must carry weight. Of the more modern methods of diagnosis by way of reaction to various forms of tuberculin, the plan of inunction, as advocated by Moro, is regarded as the best. The ophthalmic reaction is held to be by no means free from risk, while inoculation by injection is as unpleasant in practice as it is uncertain in result. Much of the book is devoted to consideration of the best means of dealing with early cases. Rest during activity of the poison is generally regarded as essential, and hence the sanatorium becomes a necessity for the successful handling of patients who are not in a position to obtain the necessary rest for themselves. Tuberculin dispensaries have been proved to be of great value in cases where rest is not essential, but the combination of tuberculin treatment and sanatorium life presents on the whole the most hopeful prospect of arrest or annihilation of the disease.

In common with other directors of sanatoriums, Dr. Thomson has studied the results of autoinoculation occurring after undue exercise, but he fully recognizes that other factors must often be at work. It is necessary to exercise extreme care in the selection of cases for induced autoinoculation, remembering that tolerance may be established without any real immunity being conferred. The cautious and graduated use of heteroinoculation may be called for, and the most minute instructions are given for the guidance of those who wish to use it in general practice.

Public measures for the protection of the community are now taking shape, and co-operation is greatly to be

[2] *Consumption in General Practice.* By H. Hyslop Thomson, M.D., D.P.H., Medical Superintendent, Liverpool Sanatorium. Oxford Medical Publications. London: Henry Frowde and Hodder and Stoughton. 1912 (Med. 8vo, pp. 350; charts 28. 12s. 6d. net.)

desired, but the ultimate stamping out of the disease will not be secured until the infective case is brought under control. The promiscuous output of bacillus-bearing excreta must be penalized, since the disregard of precaution by even a single person with a disintegrating tuberculous lung must always constitute a danger to his surroundings both at home and abroad. Dr. Thomson's work covers the whole field of morbid conditions which may be comprised under the broad title of consumption, and his general observations on the subject of treatment are sound and suggestive. His remarks as to unsuitable cases and on after-care should be specially noted. An appendix on sanatorium construction contains much interesting information as to cost of building and maintenance.

SURGICAL TUBERCULOSIS.

IT is rare in these days to find sixteen years elapse between the appearance of the first and the second edition of a medical book. Sir W. WATSON CHEYNE'S work on *Tuberculous Diseases of Bones and Joints*[3] was first published in 1895, and he says that in preparing the second edition he has been much struck by the alteration which has been taking place in connexion with the question of operative treatment. There is little that is new in the portion of the work dealing with pathology and symptoms. In discussing tubercle as an hereditary disease, the author thinks that we must not be too rigid in exclusion of the possibility of direct transmission from parent to child. He instances the transmission of the *Spirochaeta pallida* to the offspring as an example of the direct transmission of the virus itself. And, if direct transmission of one organism may occur, why may not that of another? If the bacillus gains access to the ovum, will it at once begin to grow and destroy the ovum, or will it soon be destroyed by the actively growing cells? Post-natal infection may be a much more easy explanation, but the author confesses that he cannot divest his mind of the suspicion that something of the same kind of process takes place in tuberculosis as occurs in syphilis, and that we are going too far in attributing all cases to infection after birth, and in looking on heredity as only an inheritance of a special susceptibility to the attack of the tubercle bacilli. There is, he thinks, quite a possibility that the bacilli are also inherited, but may remain dormant for a long time till circumstances arise which increase the susceptibility of the body.

In discussing the part played by injury in determining a tuberculous lesion, the author points out that in those cases in which an injury is blamed it is usually a slight injury, such as a sprain or a blow. It is a matter of common knowledge that severe injuries in tuberculous subjects do not lead to the formation of tuberculous foci at the injured part. The explanation of this may be that in violent injury, such as fracture, there is so much reparative material produced, and the process of repair is so vigorous, that the bacilli cannot obtain a foothold; whereas slight injuries simply disturb the nutrition of the part, or at most set up the early stage of inflammatory action, and thus produce conditions favourable to development of the organism. The author investigated these questions some years ago, and he gives statistics which show clearly that injury plays a part which is not merely accidental, but that it is an active, exciting cause of the disease. Not only so, but in those cases in which it was known that there had been a definite injury, it was observed that the disease assumed a more serious form, that suppuration was more prone to occur. A larger range of statistics than those at the author's command may give more definite information as to the relationship between injury and the occurrence of tuberculous disease. The matter is of great importance in view of the questions which the Workmen's Compensation Act opens up in relation to the remote effects of injury. A due proportion of space is given to the discussion of treatment. Sixteen years ago operations were performed frequently and early in the hope of eradicating the disease; now careful hygienic and local treatment give superior results. The author fears that there is a tendency for conservative measures to be sometimes unduly prolonged. The chapters on treatment are full of the most valuable critical matter. All the methods have been thoroughly tried by the author, and he has had the advantage of investigating the cases treated by Lister and himself by the older methods. The result is that all methods of treatment are approached with an open mind, and the good that appeared in the older methods is preserved, while a temperate enthusiasm is shown for the new. It will interest readers to know that the author advocates the use of the actual cautery and other forms of counter-irritation in some cases, with the object of improving the local condition and getting rid of chronic inflammatory products around the tuberculous lesion proper.

Tuberculin has not proved a success in the author's hands, and Bier's congestion method is commended with some reserve, indeed with hesitation. The use of iodoform, its action on the tissues, the injection of iodoform emulsion, and the injection of bismuth paste, all receive due attention. More might, perhaps, have been made of the diagnostic value of injection of bismuth paste in showing in a skiagram the extraordinary ramifications of sinuses in tuberculous bone disease. The chapter on operation as against expectant treatment is well thought out. How to decide between the two plans is not teachable in books, so much depends on the condition of the patient, his surroundings, his future chances in life, the experience of the surgeon. The author is struck with the greatly lessened need for operation in these days; he thinks that it may be due to better hygienic conditions in which the people live, and also, perhaps, to diminished virulence of the disease.

The work is one which we most heartily commend to the notice of the profession; the family practitioner will find it an immense help in dealing with cases which come under his charge in all stages and remain in his care throughout, and the surgeon will find well-considered, settled opinions, together with full descriptions of apparatus and operations.

[3] *Tuberculous Diseases of Bones and Joints: Their Pathology, Symptoms, and Treatment.* By Sir W. Watson Cheyne, Bart., C.B., F.R.S., F.R.C.S., D.Sc., LL.D., etc., Professor of Clinical Surgery at King's College Hospital, Senior Surgeon to King's College Hospital, etc. London: Henry Frowde and Hodder and Stoughton. 1911. Oxford Medical Publications. (Med. 8vo, pp. 415; figs. 170. 16s. net.)

JOINT DISEASES.

IN a volume on static joint diseases[4] Dr. GEORGE PREISER has amplified the theme dealt with in his small book on arthritis deformans coxae and variations in the position of the acetabulum, which was reviewed in 1908 (BRITISH MEDICAL JOURNAL, 1908, vol. i, p. 873, April 11th). Dr. Preiser, together with Drs. Stern and Lackmann, has charge of an orthopaedic institute in Hamburg, a kind of private hospital which has always been popular in Germany but which is practically unknown in Great Britain. There he has had the advantage of close study of a number of cases of chronic joint trouble, and the results of his extended experience are to be found in the present work, in which he deals with statical incongruities and consequent arthritis chiefly in the lower extremities, and devotes only a small space to diseases of the upper extremities. As in his view, rickets is the great cause of abnormalities of the joints, so it is indirectly a frequent cause of arthritis. In the rickety pelvis, as Dr. Preiser maintained in his previous book, the centre of the acetabulum is generally situated in front of Nélaton's line, and as a consequence the tip of the great trochanter is above the line. At the same time the acetabulum looks more forwards than normal, and in relation to its axis the femur is rotated outwards, so that in an extreme case only half of the articular cartilage of its head is in the cotyloid cavity. This is an insecure position, and to render it more secure the pelvis is tilted and secondary lordosis follows. The abnormal stresses thus set up often result in arthritis deformans coxae. That deformities of the femur and leg and foot bones and of the joint surfaces, whether the result of disease or of injury, also cause arthritis Dr. Preiser now endeavours to show by the evidence of a number of case-histories, of outlines taken from *x*-ray pictures, and from reproductions of radiographs. Some of the outlines are so remarkable that it would have been interesting to see the original radiographs from which they were taken. Whether we accept Dr. Preiser's theory or not, there is no doubt of the justice of his claim that by persevering and discriminating

[4] *Statische Gelenkerkrankungen.* Von Dr. Georg Preiser, Hamburg. Stuttgart: Ferdinand Enke. 1911. (Sup. roy. 8vo, pp. 286; Abbs. 272. M. 10.)

treatment, such as is carried out in his institute, a great deal can be done for cases of chronic arthritis, both to improve function and to lessen pain, and that the pessimistic attitude too often assumed by practitioners is not justifiable. The author says that Ernst Walkhoff has been studying the subject independently from a pathological point of view, and has arrived at results which bear out his theory. Whatever conclusion the reader may come to, he will find much interesting and instructive matter in this book.

The sole object of Dr. EDUARD WEISZ's book on the physical therapy of joint diseases[5] is to bring modern methods of treating joint diseases briefly and intelligently before students and also practitioners. His twenty years' experience at the well-known Hungarian baths of Pöstyén enables him to do this with some authority. Of the first twenty chapters, eighteen are devoted to concise descriptions of the various means of treatment, including the sick-room. The author's devices for extemporizing traction apparatus for the treatment of contractures in patients who cannot avail themselves of institutional treatment are good and simple, and such as any general practitioner should be able to rig up for his patients. Gymnastics, massage, and Swedish exercises are shortly dealt with, as are the various forms of electrical treatment, radium, local heat and cold, and air and sun baths. Diet is also considered. Serum and vaccine treatment in joint disease he does not strongly advocate, but thinks it is often an *ultimum refugium*. He recommends Bier's methods, especially for their power of relieving pain. With operative measures, such as excision and erasion, the book does not deal. The six chapters of the special part of the work are concerned with congenital joint disease, new growths, diseases of nervous origin, trauma, infection, and constitutional joint disease caused by altered metabolism, such as arthritis deformans, haemophilia, gout, and metallic poisoning. Altogether, this should prove a handy little book for readers of German.

NOTES ON BOOKS.

IN writing his elementary handbook of the anatomy and physiology of the nervous system, Dr. J. D. LICKLEY[6] has kept in mind the needs of students of psychology and neurology. These students form a growing body; they have not, as a rule, been grounded in the associated sciences—such as zoology and chemistry—as the ordinary medical student has. Hence their need for a special introduction to the study of the nervous system, written on broad and elementary lines, simpler, yet more extensive than the many textbooks that describe the nervous system as the medical student needs to know it. Dr. Lickley has succeeded in writing a compact and clear account of what the psychological student should know about the central and peripheral portion of the nervous system. His style is lucid, and sufficiently dogmatic for the purpose. Plenty of illustrations are given, mainly borrowed from the standard works of Gray or Quain or Testut. No idea of the magnification employed in drawing many of these figures is given, and this, we cannot but think, is an omission that should be corrected. The student who is led to expect a medullated nerve fibre to be as stout as a pencil—as it is in Fig. 14—is bound to feel that he has been deceived when he discovers how much smaller it is in real life. Dr. Lickley's book may be warmly recommended to the attention of those students for whom it is designed.

The large majority of books for the guidance of students in chemistry follow much the same lines, and any one which leaves the beaten track deserves to be welcomed. In his *Problems in Practical Chemistry*[7] Mr. G. F. HOOD has brought together a series of exercises, suitable for somewhat advanced students, which are not a mere repetition of matter to be found in other books. Each problem is set forth with any information bearing upon it which it is necessary for the student to possess, but the method of solving it is left to his own devising; the time which ought to suffice for the investigation is stated, and in another part of the book directions are given for the assistance of those who find themselves unable to arrive at a suitable method without it. The problems appear to be well chosen and the information of a practical and useful character; the book will prove very useful to students working with but little guidance from a teacher.

With what race or nation is to be the future of the world? Is the white man to hold the balance of power, or are the yellow hordes of Asia, awaking, like Japan, from the sleep of centuries, to destroy his predominance? And if a white race is to rule, which is it to be? Such are the questions Mr. FRANK FOX attempts to answer in *Problems of the Pacific*.[8] The Pacific, he says, is the ocean of the future; the scene of the next great struggle for supremacy. Britain with India, South Africa, Australia, Canada, and her other possessions washed by its waters has paramount interests there; whilst America, having made a way for her merchant ships as well as her ironclads through the Panama Canal, and having provided a great naval base of immense strategic importance at Honolulu, has made sure her position in the great ocean. Mr. Fox comes to the conclusion that no yellow race—not even the teeming myriads of China—can compete on equal terms with the white man. In all the great colonies not only are there barriers to the entrance of Asiatics, but the military spirit, the sentiment of imperialism, is strong and active. The rise of Japan calls forth the comment that she has reached the zenith of her achievements. Mr. Fox's book is well worth perusal. It presents in readable form an exposition of a problem in which every one has an interest.

A small book on the management of skin diseases,[9] by Dr. OSKAR SCHEUER, consists of two parts. In the one he gives an explanation of the general action of the remedies commonly used in dermatology, and in the other supplies succinct descriptions by which each different skin disease may be recognized, endeavours to account for their existence, and suggests the lines of treatment that should be followed. The way in which different remedies should be employed is carefully described, and the book is so arranged that information on any point can easily be found, so that it should be found useful by medical men in whose practices cases of skin disease occur with comparative infrequency.

In a small book on the outlines of mechano-therapy, Staff Surgeon Dr. FRITZ SCHOLZ[10] gives a useful and practical account of the principles of massage in general, of its application to various parts of the body, and of its practice in various diseases. He points out, however, that the number of pathological conditions in which massage alone is capable of working a cure is very small. In most cases gymnastics and exercises must be combined with massage. Under gymnastics Dr. Scholz includes sports of many kinds. Speaking of cycling, he makes the interesting remark that it is a well known fact that since it became so common the number of cases of heart disease in those doing military service is disproportionately increased. Cycling is, however, useful under proper limitations. Riding, rowing, swimming, mountain climbing, ski-ing, dancing, and lawn tennis, all are useful in moderation, but Dr. Scholz bans football because it usually oversteps the limits of healthy sports and leads to many accidents. Of Swedish gymnastics and gymnastic machines, such as the Zander and Herz apparatus, a concise account is given. The sections on special applications to different diseases are good, notably those on nervous diseases (for example, ataxia) and on joint and muscle disorders. This book is well and fully illustrated, and should be very useful to those who can read German.

[5] *Die physicalische Therapie der Gelenkkrankheiten für Aerzte und Studierende.* Von Dr. Eduard Weisz, Budapest. Berlin and Wien: Urban and Schwarzenberg. 1912. (Crown 8vo, pp. 273; Abbs. 83. M. 6.)

[6] *The Nervous System.* By J. D. Lickley. London: Longmans, Green, and Co. 1912. (Fcap. 4to, pp. 142; 118 figures. 6s. net.)

[7] *Problems in Practical Chemistry, for Advanced Students.* By G. F. Hood, M.A.Oxon., B.Sc.Lond. London: Mills and Boon, Ltd. 1912. (Crown 8vo, pp. 270; 22 diagrams. 5s. net.)

[8] *Problems of the Pacific.* By Frank Fox. London: Williams and Norgate. 1912. (Demy 8vo, pp. 294; 1 map. 7s. 6d. net.)

[9] *Taschenbuch für die Behandlung der Hautkrankheiten für praktische Aerzte.* Von Dr. Oskar Scheuer. Berlin and Vienna: Urban and Schwarzenberg. 1911. (Post 8vo, pp. 157. M. 4.50.)

[10] *Grundriss der Mechanotherapie (Massage und Gymnastik) für Studierende und Aerzte.* Von Dr. Med. Fritz Scholz, Stabsarzt im ersten Garde-Regiment zu fuss. mit einem Vorwort von Geheimrat Professor Dr. Brieger. Jena: Gustav Fischer. 1910. (Sup. roy. 8vo, pp. 157; Abbs. 37. M. 4; geb., M. 4.80.)

THE Hoffmann-La Roche Company (7 and 8, Idol Lane, London, E.C.) is now in a position to forward upon application a booklet containing abstracts of various papers dealing with omnopon from clinical and other standpoints. One of them is by Professor Sahli of Berne, the originator of the preparation, and another by Dr. H. M. W. Grey, of the Royal Infirmary, Aberdeen.

MEDICAL AND SURGICAL APPLIANCES.

Splint for the After-Treatment of Osteotomies of the Femur.

MR. R. CHARLES (House Surgeon, County Hospital, York) writes: This splint has been designed by me for the after-treatment of osteotomies in connexion with the femur, especially for Macewen's supra-condyloid operation or fractures of the middle and lower end of the femur which have been plated. It consists of a long outside splint with movable foot-piece and two small internal splints. Above

and below the knee are two parallel slits. The accompanying illustration shows the arrangement of the straps passing between the slits and buckling on the outside and not passing over the splint. They are thus in close contact with the leg, holding the broken ends firmly. I have used it in many osteotomies and fractures, giving excellent results. I find it a very useful splint if the wound requires a dressing or stitches to be removed. This can be carried out without interfering in any way with the complete immobilization of the limb. It is cheap and easy to apply, and can be made by any hospital carpenter.

A Hand Perimeter.

Dr. R. BEATSON HIRD (Ophthalmic Surgeon to the City of Birmingham Education Committee) writes: A perimeter should be of more than passing interest to those outside the ophthalmic speciality, but its cost hitherto has been prohibitive. The perimeter which Messrs. Dixey and Son, of Bond Street, London, have made for me is a modification of a pre-existing hand perimeter. It is simple in structure with no mechanism to get out of order. The handle is detachable, and the whole can be carried in an ordinary handbag. It is light, so that any patient can easily hold it in position. There is a rotating half arc enabling the field to be mapped out rapidly, the dial registering the part of the field that is being tested. The face-rest is made of vulcanite for the purpose of being cleansed, and the fixation object is a white spot 10 mm. in diameter, so that it can be seen by presbiopes. The field is tested by white and coloured squares mounted on black handles which are carried along the arc. A book of 100 charts is provided. The fields can be mapped out accurately and quickly for white and colours, and with it the fields of a patient in bed can be taken if desired. The cost of the perimeter is 17s. 6d. and the book of charts 3s. 6d. The workmanship is excellent.

Improvised Valve for Empyema Tube.

Dr. JOSEPH H. CHURCHILL (Wembley, Middlesex) writes: I have recently affixed a valve to an empyema tube in such a way as to impede the ingress of air on inspiration, yet not obstruct the egress of air or pus. The effect in the case of a boy of 16 has been that his respiration frequency, which for some days after the operation was 28, dropped on the first night after inserting my home-made tube to 18, and has remained about the same since. I think that the lung has expanded much more readily in consequence of the valve, whose action appears to make each inspiration of increased effect in that direction. The valve is made as follows: To an ordinary rubber empyema tube with flange a small square of thin rubber sheeting is affixed by one of its edges by four Michel's sutures to the surface of the flange so as to cover the opening. On each side a section of stout rubber drainage tubing is stitched to act as a pad to prevent the dressings from pressing on the valve. The flange should be at least 2 in. broad and 3 in. long, so as to cover the wound completely and lie close to the skin well outside its limits. At first the valve was affixed by means of tyre-patching solution, but hot lotions were found to render this method of attachment valueless.

EXPERIMENTS ON ANIMALS.

IT would tax the assimilative powers even of those who, in Lord Rosebery's phrase, "can lunch on a biscuit and a bluebook" to digest the evidence, "volumed and vast and rolling far," given before the Royal Commission on Vivisection. As each of the four volumes of minutes appeared we summarized the evidence with the greatest care to omit no point, on one side or other, that seemed to be important. These summaries, if collected, would have made a goodly tome which none but persons keenly interested in the subject would care to attack. Yet unless the evidence is closely studied no conception can be formed of the contrast between the statements of the scientific men on the one hand, and the antivivisectionists on the other, and the spirit and temper and, we may add, regard for truth displayed by each side respectively. Mr. Stephen Paget has now put the gist of the evidence before the public within manageable compass.[1] He states in his preface that the book was written at the request of the Committee of the Research Defence Society. All of it, except the introduction and a final chapter in which the findings of the Commissioners are reviewed, was seen through the press long before the Royal Commission on Vivisection published its belated report.

In the introduction Lord Cromer sets forth his reasons for accepting the post of President of the Research Defence Society some four years ago. He abhors cruelty to animals, and, as is well known, that feeling made him take active steps in Egypt to prevent such cruelty. Before associating himself with the Society he fully satisfied himself both that the main accusations brought against the experimentalists were wholly devoid of foundation, and, further, that the men with whom he was to work were animated by a hatred of anything like wanton cruelty not less profound than his own. He felt strongly that the vivisectionists, and not their opponents, were the true humanitarians, and that they were, in circumstances which rendered them peculiarly liable to misrepresentation, fighting a cause in which not only the whole human race, but also the brute creation, were deeply interested. Since the issue of the Report of the Royal Commission the consciences of lovers of animals may be at rest; they have the deliberate pronouncement of a body of men of unquestionable ability and impartiality, who gave the most careful attention to the evidence for and against vivisection. Lord Cromer said in a letter addressed to the press immediately after the issue of the Report that, "broadly speaking, the supporters of vivisection have proved their case." A more careful study of the Report has confirmed him in this conclusion, and he proceeds to give a summary of his reasons. He first deals with those who hold that "nothing less than the total prohibition of all experiments on animals will satisfy them," and thinks that the conclusion of the Commission against this view will be approved by general opinion. Further, the Commissioners express the opinion that valuable knowledge has been acquired in regard to physiological processes and the causation of disease, and that useful methods for the prevention, cure, and treatment of certain diseases has resulted from experimental investigations upon living animals. Dr. George Wilson, a member of the Commission, who greatly dislikes vivisection, subscribed to these statements with certain reservations, holding that the useful results claimed have been enormously overestimated, and that "the fallacies and failures are far more conspicuous than the successful result." Lord Cromer points out that in this matter most people can only to a limited extent form their own opinions on the facts and statistics laid before them, and must in the main rely on authority. The doctors who share the views expressed by Dr. Wilson are a very small minority, and it may be added that there is not one among them whose opinion on a scientific point carries weight with the profession. That the results of experiments have sometimes proved fallacious or useless is no argument against the method. The progress of science is slow, and much of the work by which it is advanced is in the nature of groping in the dark, or following more or less plausible hypotheses

[1] *For and Against Experiments on Animals.* Evidence before the Royal Commission on Vivisection. By Stephen Paget, F.R.C.S., Honorary Secretary Research Defence Society. With an Introduction by the Right Hon. the Earl of Cromer, O.M., G.C.M.G., G.C.B. London: H. K. Lewis, 136, Gower Street, W.C. 1912. All rights reserved.

which may turn out to be will-o'-the-wisps. In science as well as in practical things it is true that the man who never makes mistakes will never make anything else. How large a part happy accident has played in discovery would form a fascinating chapter in the history of science. Yet the fact that discoveries have been made by accident, and the other fact that apparently well-planned investigations have led to nothing, does not invalidate the general proposition that it is only by the experimental method rigidly applied that progress can be made. Of science it may be said as the ancients said of Nature, *non facit saltum*. Scientific truth is doubtless, as Huxley said, largely built on scientific error. The special value of the experimental method is that it enables us, if we may paraphrase a saying of St. Augustine, to make of the very errors that are crushed by further research a ladder to the attainment of truth.

In dealing with the question of pain, Lord Cromer takes occasion to define the attitude of the Research Defence Society on this point. Its view is not that pain is never inflicted, but that the vast majority of inoculations involve no pain or none that may not truly be called trivial. It maintains that all experiments involving operation are conducted under an anaesthetic, and that the anaesthetic is effective; that the law already provides safeguards against the unnecessary infliction of pain; that if any additional safeguards could be suggested which would not be unduly or vexatiously restrictive, they should be sympathetically considered; and, finally, that in those rare instances where some pain is inflicted, the results justify its infliction. One has only to read the evidence of the antivivisectionists to agree with Lord Cromer that on this vital point their case, when submitted to the test of cross-examination, completely broke down. Lord Cromer does not think that the adoption of the recommendations of the Commissioners[2] in the direction of further restrictions would seriously hamper scientific investigation. Much, of course, would depend on the manner in which they are carried into effect. Before any legislation, therefore, is undertaken, or new regulations framed, he says the advocates of vivisection should be taken into counsel and should have every opportunity of expressing their opinion. Dealing especially with experiments on dogs, he points out that with every allowance made for sentiment, he would be sorry to see dogs altogether excluded, as he cannot doubt that by such exclusion the cause of science would suffer. He gives instances of the good that has been done to dogs themselves by experiment, instancing Dr. Copeman's discovery of a vaccine against distemper, and the more successful treatment of malignant jaundice in dogs. He is of opinion, however, that the proposal made by the majority of the Commission to the effect that the legal position of dogs should be assimilated to that of horses and mules, might reasonably be adopted; that is to say, a special certificate should invariably be required whether anaesthetics are used or not.

Lord Cromer concludes with an appeal to the main body of his countrymen and countrywomen not to allow a misplaced sentiment to misguide them, but to study the facts for themselves by the light of the information now placed at their disposal. Should they do so they will, he thinks, come to the conclusion that, under proper safeguards, not only should vivisection be allowed to continue, but that its arrest would be disastrous to the further progress of humanitarian science in this country.

Mr. Paget gives the text of the Act of 1876, pointing out that no mention is made in it of inoculations because bacteriology was then in its infancy. Ninety-five per cent. of all experiments on animals in this country at the present time are inoculations or similar experiments, none of which involve any sort or kind of cutting operation on any animal, but owing to this fact every inoculation and every similar experiment has to be scheduled under Certificate A. as an experiment performed without anaesthetics. Of this the antivivisectionists avail themselves as a proof that "torture" is legalized. As Mr. Paget points out, however, Certificate A is never allowed for experiments involving a cutting operation. A summary of the evidence before the Royal Commission is then given with a running commentary. An analysis of the Report of the Commission concludes the book. Some defects in the Report are referred to. Of these the most important, in our opinion, are that it does not emphasize the difference of quality between the antivivisection evidence and the evidence of men like Lord Rayleigh and Sir R. Douglas Powell, and that the Report being four years behind the times necessarily says nothing of many discoveries which are of the highest importance. Notable examples of this are the remarkable success achieved at Panama, and the saving of life in tropical climates which has resulted from the adoption of the experimental method. Against these defects must be placed the important fact that the Report is unanimous. Whatever reservations or minimizings are set forth in memoranda by Sir William Collins, Colonel Lockwood, and Dr. George Wilson, they have appended their names to a report which admits that "valuable knowledge has been acquired in regard to physiological processes and the causation of disease, and that useful methods for the prevention, cure, and treatment of certain diseases have resulted from experimental investigations upon living animals." It is further stated that it is highly improbable that without experiments made on animals, mankind would at the present time have been in possession of such knowledge; that, in so far as disease has been successfully prevented, suffering has been diminished in man and in lower animals, and that there is ground for believing that similar methods of investigation, if pursued in the future, will be attended with similar results. How in the face of the fact that such a report is signed unanimously by the Commissioners, the antivivisectionists can profess to believe that the Report is in some way a triumph for their cause, is interesting as a revelation of their mental constitution.

Mr. Paget has done signal service by placing the results of this elaborate and prolonged inquiry before the profession and the public in a convenient form, with just the amount of commentary that is required to bring out the true bearings of the evidence and the value to be attached to the testimony of the witnesses on either side.

[2] BRITISH MEDICAL JOURNAL, March 16th, p. 623 et seq.

THE PROCEEDINGS OF THE INTERNATIONAL OPIUM CONFERENCE AT THE HAGUE.

II.*

Report of Redaction Committee.

THE Conference resumed its sessions after the Christmas recess on January 9th, but the Redaction Committee had been at work previously, and had, under the Chairmanship of M. Brenier (another French delegate), who had taken the place of M. Guesde, produced an elaborate and lucid report. This report dealt (1) with questions of form—polishing the resolutions, as passed, into articles of Convention, and under the guidance of M. Asser eliminating inconsistencies and redundancies; (2) with certain questions which exceeded mere matters of form and on which the Report of the Committee discloses sharp divergence of opinion on the part of some of its members, and upon which (while reserving the liberty of their respective delegations) the members of the Committee found it necessary to seek the decision of the full Conference. The division of opinion appears to have centred around the question of the coming into force of the Convention. Only twelve nations were represented at the Conference. Others had been invited, but had not accepted the invitation, while yet others had not been invited to participate. Among the nations not represented were several which, either actually or prospectively, had an important stake in the decisions which might be arrived at. Moreover, the inclusion of morphine and cocaine, by widening the subject-matter of international agreement, had extended the geographical area involved in the operations of international engagements. What was a Far Eastern problem, or appeared to be so when the Shanghai Commission was initiated, had become an affair of world-wide importance before the Hague Conference had got half through its labours. Clearly, for a few nations to bind themselves by a self-denying ordinance to restrict trade in opium, morphine, etc., while leaving others free to extend their commerce in the like commodities, or even to set up a trade which the more virtuous had either abandoned or severely limited, would be neither just nor politic—neither

* The first article appeared in the BRITISH MEDICAL JOURNAL of August 31st, p. 503.

good business nor good law. On the other hand, it appeared that there were in all some thirty or more sovereign Powers unrepresented at the Conference, to all of whom these invitations to sign the Convention would have to be addressed if the international engagement was to be universally applicable. Diplomacy could not tolerate any picking and choosing in the dispatch of any such invitations, though it was obvious that the non-represented Powers included States very diversely related to the subject of the Convention. Thus, amongst the unrepresented Governments were Turkey, Austro-Hungary, and Spain; also Bulgaria, Bolivia, and Peru, as well as Guatemala, Cuba, and Costa Rica. Then, again, there was the case of the colonies, dependencies, protectorates, and leased territories of some of the Powers concerned. Such world-wide expansion of the work might have appeared wellnigh overwhelming, or might even serve indefinitely to delay the coming into force of the results of the Conference, and thus play into the hands of those who desired that the trade in these noxious drugs should remain unmolested and unrestricted.

The Redaction Committee in their report set out in detail the foregoing considerations, thus briefly summarized. They pointed out that the normal course in the case of similar Conventions was for the Governments represented at the Conference to proceed to ratification as soon as possible after the plenipotentiaries had signed the original draft. The deposit of ratifications with the Netherlands Government would then be immediately followed by an invitation to the non-signatory Powers to adhere to the Convention. This course, indeed, was the one which seemed to commend itself to the Conference before Christmas, but with an amendment on the part of the British delegation proposed by Mr. Max Müller. This amendment provided that so much of the Convention which did not involve legislation should come into force upon ratification by the signatory Powers. Legislative enactments as distinguished from administration orders were to be introduced to the respective parliaments as soon as possible, but such laws should only become operative as from a date to be mutually agreed upon.

It appears, however, from the Redaction Committee's report that at its meetings an entirely new proposal had been put forward by M. Delbrück (Germany), whereby ratification was to be subordinated to adhesion, and that ratification, and *a fortiori* the coming into force of laws flowing from the Convention, were to be postponed until every non-signatory Power in Europe and America had "adhered" not only for themselves, but also for their colonies, protectorates, and leased territories. The report proceeds to say that, on the other hand:

Sir William Collins had contended (in the Committee) that the subordination of ratification to the adhesion of the other Powers, and especially if it be enacted until *all* other Powers have adhered, would indefinitely postpone the practical application of those measures to which the contracting Powers had, by their presence and the successive votes of their plenipotentiaries at the Conference, given their approval. The work of the Conference would thus assume a character which would be liable to be misunderstood. The British delegation was obliged, no less than the German, to have regard not so much to the ratifying power (rôle) of its Parliament as to its opinion.

On January 11th, at the seventeenth session of the full Conference, the sharp division of opinion thus disclosed in the Report of the Committee came under discussion.

M. DELBRÜCK said:

The proposal to subordinate ratification to adhesion made it necessary to make some general remarks on the question of ratification. The act of ratification had a very different value for the different States represented here. In England, Japan, and Russia ratification is effected without the intervention of the legislative power. In other countries the Government is obliged to submit the Convention to Parliament prior to ratification. That is the case in Germany; but it is not only that. In Germany a ratified treaty has the force of law; it is promulgated as such in the *Official Gazette*, and is binding on all State authorities and tribunals. In a certain sense a ratified treaty has greater force than a law, since it cannot be changed merely by further legislation. The law courts will not acknowledge a law which is in contradiction to an international treaty. That is not the case with all the Powers represented here. In the United States, for example, jurisprudence has formed a totally different conception of the binding force of an international treaty. The Supreme Court has repeatedly ruled that a ratified international treaty is subordinate to internal legislation. The treaty only becomes binding on the law courts as the result of internal legislation which may or may not follow, and such laws are liable to revision and amendment by fresh legislation not necessarily in accord with the treaty.

He therefore asked the American delegation what guarantee they could give to the other Powers that, after ratification, their Government would pass the legislation necessary to put the stipulations of the treaty in force.

Dr. HAMILTON WRIGHT replied that:

The United States could give a guarantee of one hundred and twenty-five years of national good faith. A convention or international treaty was ratified with the consent of the Senate. His own understanding was that when once a treaty was ratified it became the law of the land. If it were ratified by the Senate and approved by the President it would become law. If the United States undertook to pass or to propose legislation to enforce any article in the treaty, that legislation would be passed or proposed.

Mr. CREMER drew attention to the difficulty of distinguishing between legislation and administration as contemplated by the British amendment introduced by Mr. Max Müller and to which Sir William Meyer had directed attention. In their Far Eastern colonies several of the Powers promulgated by administrative orders what was equivalent to legislative enactments; such distinction therefore could not be sustained.

Sir WILLIAM COLLINS explained that the so-called British amendment had to do with the former proposal of M. Delbrück, in which the normal course of ratification first and adhesion afterwards was prescribed.

But the whole position was altered by M. Delbrück's new proposal which reversed the usual order. What they all desired was to see practical effects flowing from the Conference; matters of form were of less importance. He gathered that it was now intended by M. Delbrück that *every* other Power in Europe and America must adhere before ratification could take place. The proposal to make adhesion precede ratification was unusual, and, according to the Committee's report, unprecedented. They were accordingly confronted by a new situation. If it would make for celerity and progress, he would be inclined to agree with His Excellency M. Asser that there was no objection in principle, but the British delegation were in communication with their Government on the point.

M. BRENIER said he, too, had to ask instructions from his Government.

M. DELBRÜCK said the Conference itself was unprecedented, since only twelve Powers were represented.

Sir WILLIAM COLLINS pressed M. Delbrück for a reply to his question as to whether he wished ratification to be delayed till every Power had "adhered."

M. DELBRÜCK replied he could hardly answer that. He thought all the Powers should be invited. Perhaps if some of them stood out, those who were ready to act would say they would proceed without them.

Sir WILLIAM COLLINS said:

As you have drafted your proposition it appears that ratification must wait until *every* other Power has come in.

M. DELBRÜCK said:

If it is seen that all the Powers answer that they are ready to come in, we will have the ratification.

Sir WILLIAM COLLINS said:

If I am to understand that M. Delbrück does not intend us to wait for *all* the Powers to come in, I think some alteration in the wording of his proposal will be required.

At the next sitting a motion was made by M. BRENIER, on behalf of the French delegation, referring the matter back to the Redaction Committee with an instruction to submit at the next meeting a definite scheme for ratification, adhesion, and coming into force (*mise en vigueur*) of the Convention, and this motion, with an amendment (carried by 6 to 5) proposed by the American delegation which further instructed the Committee, in so doing, to have regard to all proposals made, but at the same time precluded them from expressing preference for any one of them.

Sir WILLIAM COLLINS said:

He felt sure his colleagues on the Committee would agree with him in saying that they were greatly honoured, and he might add embarrassed, by the additional labours and trust which the Conference seemed determined to place upon them. They will be required to present at the earliest possible moment a formula as to the principle of which the Conference had not vouchsafed any opinion. When the British amendment had been proposed they all assumed that the ordinary course of ratification, followed by adhesion, would be adopted. They separated for the holidays without a whisper of a suggestion that there would be any deviation from the usual precedent;

but when they returned they were confronted with a new situation which had taken them by surprise. It would have been an advantage to the British delegation if they had received earlier intimation of the proposal to render ratification dependent upon the adhesion of every other Power. But the British delegation had always set more store upon the earliest possible enforcement of the matters agreed upon than upon any pedantic concern for form or precedent.

It appears that at the next (nineteenth) session the Committee obligingly furnished a formula dealing with these contentious questions which M. Brenier explained was a compromise arrived at with the assistance of the jurist, M. Asser, and the Dutch Minister for Foreign Affairs, and which embodied some of the features of the German, British, and French proposals respectively.

M. SAVINSKI, on behalf of Russia, approved what he called the "conciliatory mean," which had been lighted upon.

M. DELBRÜCK said:

We will use our best endeavours to get it approved by our Government; I would not like to go further now.

Sir WILLIAM COLLINS said:

The proposal which was now presented was rather composite as regards the sources from which it had been gathered, and although naturally it had not yet been communicated to the British Government, their delegates would endeavour to secure its approval. The proposal met their chief objection, namely, to obviate a single Power by saying "No" to the invitation which was to be addressed to all the Powers, blocking the way for two whole years. Also it met the case of one or two Powers out of the forty or more who would be invited to express their approval, failing to reply by December 31st, 1912. In such case those Powers which had participated in the Conference, or which had subsequently signed, would then, through their representatives, meet and decide whether they could not, nevertheless, proceed to ratify without them. It also met the other point to which the British amendment was directed, and would in the case of new laws to be promulgated enable all the Powers, so to speak, to start fair. The new draft also cured the obvious defect in the second proposal made by the German delegation. The word "adhesion" disappeared altogether. The second proposal had what he might be permitted to call the inherent vice of using the word "adhesion" where it had no rightful place. By "adhesion" they understood, he supposed, the approval by certain Powers of a Convention already ratified by certain other Powers, and as he pointed out in the Redaction Committee, when he first saw the new proposal, "adhesion" to a signature was really meaningless. That difficulty was got over by the formula now propounded. He would like to bear his testimony to the good-will which animated every member of the Committee throughout their prolonged sittings.

The form prepared by the Committee was accordingly adopted by the Conference.

Trade in Morphine and Cocaine.

This obstacle having thus been successfully surmounted, the Conference at its next session found its progress again impeded by another scarcely less formidable one—one more directly concerned with questions of a medical and pharmaceutical nature—touching the problem of trade in morphine, cocaine, etc., to which the British Government, it will be remembered, attached the greatest importance.

Dr. GRUNENWALD, on behalf of Germany, stated that although their delegation had agreed to the articles regulating the trade in morphine and cocaine, they had since found, "in laying these resolutions before the Ministry of the Interior, that a careful examination had occasioned most serious doubts as to the practicability of carrying them out. It was found, he said, that their application would necessitate amendments of their laws, with regard to trade and commerce, of a kind that would most likely not be approved by their State Legislatures." The Imperial Government apparently was unwilling to commit itself on questions which by the German Constitution are largely within the competence of the federal legislative bodies. From Dr. Grunenwald's remarks it would appear that in giving their assent to this portion of the Convention before Christmas they had assumed that their existing pharmacy laws covered the proposed regulation of the morphine and cocaine trade, and that Germany was merely agreeing to assist other nationalities to level up their regulations to the "strict and severe" laws of the Fatherland. On their return to Berlin the delegates had, however, discovered that the unanimous resolve of the Conference rigidly to restrict the home and foreign trade in these alkaloids and their preparations had outrun the legislative powers at present in existence in some, at any rate, of the constituent States of Germany. A certain jealousy naturally existed between the powers of the Reichstag and the State Legislatures, and Germany being then in the throes of a general election the time might not have been very opportune for raising questions of this nicety, especially in regard to large home industries. This being the case, Dr. Grunenwald desired to secure amendments in what had been already passed, so as "to eliminate the categorical expressions used in the articles (10–16) and replace them by expressions more suitable to the particular conditions of the German Empire"; in other words, to indicate that the Imperial Government would use its best endeavours (*nach Möglichkeit darauf Bedacht nehmen*) to execute the stipulations in question, but not so as to bind it to legislate if difficulties arose with the Federal parliaments.

Mr. MAX MÜLLER said:

They must all be very grateful to the German delegation for the clear exposition they had given, through Dr. Grunenwald, of the constitutional difficulties which prevented them, to the very great regret of the British delegation, from accepting, as they originally understood the German delegation would accept, the articles mentioned, in the form in which they had been first agreed on in private conference between the German and the British delegations, next passed by the Redaction Committee, and finally read for the first time before Christmas by the whole Conference.

He added that:

His Britannic Majesty's Government very strongly deprecated the German amendments, though they ultimately gave them discretion to meet the new situation in what they considered the best way.

They could not certainly second these amendments, though they might not oppose them if the other delegations were prepared to approve them. He reiterated his regret that there should be any weakening of the sense of the original articles, and invited an expression of the views of the other delegations.

The Conference adjourned for consultation, and then the German amendments were voted upon in succession. In most cases the record as given in the *procès-verbaux* was—seven *for* the German amendments and *four* "reserved," the reserving Powers being the United States, China, Persia, and Siam. It would appear, therefore, that the vote of Great Britain was cast in favour of the German amendments, although it is evident there was by no means unanimity on the part of the other ten Powers in its favour.

The net result of these amendments was to substitute for an undertaking to secure legislation for registration of premises, licensing of persons, inspection, as well as the regulation of import and export trade in respect of morphine, cocaine, etc., an undertaking to endeavour to do so. It was undoubtedly a weakening of the original resolution; the native hue had been sicklied o'er by the pale cast of thought. Nevertheless, two Articles remained undiluted, namely, that (No. 9) which engaged the contracting Powers to enact pharmacy laws to confine morphine, cocaine, etc., to their medical and legitimate uses, and to co-operate *inter se* for that purpose; and another (No. 14) whereby the Powers undertook to apply all such laws to medicinal opium and to all its preparations containing more than 0.2 per cent. of morphine. These Articles, as Sir William Collins has since stated (see *Contemporary Review*, March, 1912), "when put into operation, will have far-reaching results."

Granulated Opium.

Certain minor questions were discussed at the remaining sessions of the Conference. Thus the meaning to be attached to the term "granulated opium" came under consideration. Was it "raw" opium or "medicinal" opium? The term is used officially in the United States *Pharmacopœia*, but medicinal opium had been defined by the Conference as "raw opium which had been heated to 60° C., and contained not less than 10 per cent. of morphine, whether or not it be powdered or granulated or mixed with indifferent material." M. DELBRÜCK said that Germany was prepared to prohibit those forms of opium that were liable to misuse, but not to prohibit the export of forms of opium which were pharmaceutical preparations. Dr. HAMILTON WRIGHT remarked that the United States were the largest importers of granulated opium from Germany. Sir WILLIAM COLLINS said he disagreed with M. Delbrück if he thought that medicinal opium could not be put to an illegitimate use. The Technical Committee, upon instruction, offered to add to the

definition of raw opium that it should be held to include all opium containing less than 10 per cent. of morphine. This the Conference was not prepared to adopt, and no definition of "granulated" opium was included; the British delegation, however, made a declaration defining their view as to what meaning they attached to the terms "powdered" and "granulated."

Proposed Division of Convention.

The regrettable weakening of the morphine and cocaine articles, at the request of Germany, led to a proposal at the twenty-second session by Dr. HAMILTON WRIGHT, on behalf of the United States, to split the Convention into two: the one to deal with raw and prepared opium and the other with the enumerated alkaloids. Dr. Wright feared that the effect of the new articles would be to "drag to a lower plane the treatment of the opium question," and that the inclusion of morphine and cocaine in the actual form of the Convention might delay progress till the Greek kalends.

Sir WILLIAM MEYER, in reply, quoted Sir Edward Grey's dispatch, which said:

> His Majesty's Government desire to be assured that if they participate in the Conference the other participating Powers are willing that the Conference should thoroughly and completely deal with the question of restricting the manufacture, sale, and distribution of morphine, and also with the allied question of cocaine.

He did not consider there was any inconsistency in having made a concession on these articles "in order to prevent the possibility of a Great Power which had taken a prominent part in the Conference declaring its inability to accept these articles."

Dr. Wright's proposal, however, met with little support; 9 voted against it, while Germany abstained. An attempt to rediscuss the same proposal to divide the Convention at the next session was frustrated by 7 to 3, Germany, the United States, and China constituting the minority.

Conclusion of Conference.

On January 23rd the final (twenty-sixth) plenary sitting was held. The PRESIDENT (Bishop Brent) pronounced an eloquent closing address, and a graceful message from Queen Wilhelmina, who had, unfortunately through illness, been prevented receiving the delegates personally, brought the Conference to a close, the various plenipotentiaries forthwith affixing their signatures to the Convention and protocol.

INTERNATIONAL DENTAL FEDERATION.

THE twelfth annual meeting of the International Dental Federation was held in the Aula of the University of Stockholm on August 28th and 29th. Delegates from various national dental associations in Europe and America to the number of 100 attended. The opening meeting was attended by the Minister of Education and Professor Lecue representing the university. The President, Mr. W. B. PATERSON, F.R.C.S., L.D.S., of London, delivered an address in which especial reference was made to the interest taken by King Gustav and his Ministers in the establishment of medical and dental clinics in connexion with the national schools of Sweden. Reference was also made to the Federation's previous visit to Stockholm in 1902 and to a valuable statistical report which was then presented by a committee on behalf of the various national dental associations of Europe, showing the incidence of dental caries among the different peoples and revealing a high percentage of defective dentures. A comparison between that report and more recent statistical tables in the same countries demonstrated an increasing ratio of dental caries as well as a larger number of septic mouths.

Short addresses and speeches from various representatives of national dental associations also testified to the universality of the spread and injury wrought by that disease. Dr. ELOF FÖRBERG of Stockholm, who welcomed the Federation on behalf of the Swedish dental societies, dwelt upon the question of inspection and treatment of school children in Sweden.

The following also spoke on behalf of their nationalities: Professor WALKHOFF (Munich), Dr. BROPHY (Chicago), Dr. ROSENTHAL (Brussels), M. VAN DER HOEVEN (The Hague), Dr. SHMAMINE (Kioto, Japan), M. MAURICE ROY (Paris), Dr. WOLFF (Vienna), Dr. GUERINI (Naples), Professor CHRISTENSEN (Copenhagen), Dr. VALENZUELA (Chile), M. OTTESEN (Christiania), Dr. AGUILAR (Madrid), and Dr. W. GUY (Edinburgh) for Great Britain. Professor LECHE extended a cordial welcome on behalf of the university, and the MINISTER OF EDUCATION expressed his thanks. Dr. JESSEN (Strassburg) presided over the Hygiene Commission, and M. HUET (Brussels) presided over the Commission of Bibliography and Documentation.

Papers were read by Professor LAGERHEIM, of the University of Stockholm, on "Bread Stuffs," and by Dr. RÖSE, of Dresden, on "Swedish Breads and their food values," with a demonstration of the various kinds of bread in use in Sweden; Mr. HORACE FLETCHER (New York) on "An Appreciation of Modern Dentistry," in which the "cult of the clean mouth" was said to be known in some parts of the States as "Fletcherism."

A cinematographic demonstration of means to prevent the occurrence of dental caries was given by Mr. G. CUNNINGHAM, of Cambridge. The film, prepared by Messrs. Pathé Frères from actual scenes in the practice of the Cambridge School Dental Clinic, was exhibited for the first time.

It is proposed to use the cinematograph in connexion with popular lectures on the care of the teeth in various centres in this and other countries.

The Executive Council of the Federation, which consists of fifty members on a basis of proportional representation for the various countries, met to decide the award of the Miller Prize to the person who, in its opinion, had rendered the most eminent services to dentistry. The prize, which consists of a gold medal of the value of £50, a diploma of honour, and a sum of money resulting from the interest on £3,000, was awarded to Dr. Charles Godon, chief of the École Dentaire de Paris. The Miller Prize was instituted by the Federation in memory of the late Professor Miller of Berlin, the eminent odontologist, and its former President.

Considerable *éclat* was given to the meeting of the International Dental Federation by a special levée, held by His Majesty King Gustav at the Palace, of the members of the executive. Various social festivities were held in honour of the visit of the Federation to Stockholm, amongst the most important being the Hasselbacken banquet, Dr. Förberg's dinner in the Garden Court of the Royal Hotel, and the Swedish Dental Societies' dinner at Saltsjöbaden, which was attended by several distinguished personages. The weather throughout the meeting was dull and rainy, which caused the abandonment of certain boating excursions and outdoor picnics; otherwise the meeting of the Federation in Stockholm will be remembered as a great success. The Federation accepted a cordial invitation to meet in America next year.

THE EDUCATIVE FUNCTION OF THE DOCTOR.

IT has often been urged in these pages that the true way to save the people from quacks and self-drugging is to educate them as to the aims and achievements of rational medicine. Its limitations should also be indicated, for, as Herophilus of Alexandria said in a pregnant maxim, the best physician is he who distinguishes between the possible and the impossible. This is a distinction which goes to the very root of the art of healing, and it is therefore most important that its significance should be appreciated by the public. Legislation cannot compel people to refrain from self-drugging or from giving ear to the blandishments of the advertising nostrum vendor. The medical knowledge of the people, so far as there is any, is a survival of theories framed by doctors in the dark ages, and the teaching of Avicenna to his pupils—that when there is any doubt as to the nature of a disease the liver should be made the scapegoat—is still deeply ingrained in the public mind. The medical profession has an educative as well as a healing mission, and it is to be regretted that this part of its duty has been to a large extent neglected. The public insists on calling every medical practitioner "Doctor." The word means teacher, and this is precisely what doctors in their relations with their patients should be. If medical treatment is to be successful, the co-operation of the patient is necessary, and for this some

degree of comprehension of the principles of rational medicine is required.

The people in general do not read medical books or journals, or if they do so it is to their own detriment more than to their edification, for they have not the training necessary to enable them to grasp the true purport of scientific communications or discussions. But any man of ordinary intelligence can with a little trouble learn something of the nature of the fundamental problems of medicine, and what has been done towards their solution. There are not wanting popular treatises on medical subjects, but too often they are written in a style more likely to mislead than to enlighten. There was great need, therefore, of a short treatise written in language understanded of the people, setting forth briefly and simply the progress made in medical science and the stage which knowledge has now reached. This need has now been supplied by Mr. Willmott Evans in a work[1] in which he endeavours to give a simple explanation of some of the main principles on which are based the medicine and surgery of the present time. This object, in our opinion, has been successfully fulfilled. After a brief review of the medicine of the past, he gives a lucid account of the causes of disease, of the part played by germs, of the development of scientific medicine, and the use of the microscope and of the principles of bacteriology. Two chapters are devoted to an explanation of the principles of immunity, and another to a short account of vaccination. The history of the discovery of anaesthesia is well told, and antiseptic surgery and the possibilities opened up thereby are briefly but adequately dealt with. In a chapter on the value of drugs attention is called to the ever-increasing tendency to trust to the self-healing powers of the body, hygiene, and diet; but the author has a judicial mind, and does not rush into extremes. He holds that there is a solid foundation for the employment of drugs in medicine, and he thinks it probable that at the present day they are unduly neglected. He points out, however, an error into which those treating diseases are likely to fall in the use of drugs. The symptoms are mistaken for the disease, and the physician may be too ready to content himself with treating the symptoms. The layman who reads this book will learn why it is often injudicious to stop a cough, even when it is a sore affliction to the patient, and why patent medicines for cough taken without medical advice so often prove highly injurious.

In discussing secret remedies, Mr. Willmott Evans does not deny that many proprietary medicines have proved useful. It would be strange, he says, if they had not, for they contain only the ordinary drugs used in prescriptions. They are credited with special powers, because to the vulgar *omne ignotum pro magnifico*. As to the remedy, he thinks the simplest and fairest method is not to forbid by law the sale of remedies of this class, but to make it compulsory that every bottle or box should bear a correct statement of its contents.

An interesting account is given of what has been done for the prevention of malaria, one of the greatest scientific triumphs of modern medicine. Of the part played by insects in the production of disease a fair summary is given, and a brief account of industrial disease and some notions of legal medicine form the subjects of other chapters. The *x* ray, radium, and Finsen light, and their applications are briefly described, and there is a useful chapter on malingering. The book ends with a forecast of the medicine of the future, which lies in the direction of the discovery of causes. The hope is held out that many of the evils that now afflict us will in course of time be avoidable. As a result, Mr. Willmott Evans looks forward to the stretching of the span of human life several "decades" beyond the three score and ten that once were allotted to man. Should this time ever come, it is earnestly to be hoped that means not only of preventing disease, but of preserving mental and bodily vigour, will have been discovered. For if the long-lived men of the future are to be like the Struldbrugs of Swift's appalling imagination, the advance of medical science will not be a blessing, but a curse. Long life in itself is not a blessing, and it must be confessed that much as has been done for the prevention of disease, we are still far from the discovery of the secret of perpetual youth. Mr. Willmott Evans's book may safely be recommended to the readers for whom it is meant, and medical practitioners will find in it many useful hints that will help them to fulfil the educative mission which is a part of their duty.

We recognize fully the difficulties of this apostleship, but we feel strongly that it is necessary, if the people are not to be led astray by ignorance, half knowledge and mere pretension. The medical profession has been reproached with "priestcraft," and with a "pontifical attitude" in regard to what, in a scientific sense, is profane. In all this, it must be admitted, there is some measure of justice, though the charge is often absurdly misapplied. It is necessary to plead for breadth of mind on the part of the profession, and to protest against the disregard of everything outside the narrow circle of knowledge bearing the stamp of orthodoxy. We have done this in the case of cancer cures, manipulative treatment, psychotherapy, and so-called miracles. The eternal charge of conservatism has, as we have always been ready to admit, some foundation in fact, but there is a worse evil, illustrated by Mr. Gladstone's light-hearted utterance, that he kept an open mind on vaccination. To try methods on patients for the beneficial action of which there is no trustworthy evidence would be to make human beings the subjects of unjustifiable experiment.

There should be no mystery in the practice of medicine. The state of the case as the doctor sees it should be explained to the patient as far as this can be done without causing needless fears. The difficulty is that patients now do not think they are receiving anything solid in exchange for a fee unless they get a prescription. This, we fear, will be vastly encouraged by the operation of the Insurance Act. An attempt is already made to teach the elements of physiology and hygiene in schools, but from what we have seen and heard the results are not always satisfactory. Unless the elements of these subjects are taught by thoroughly competent teachers who know them at first hand, no good will be done, and possibly a great deal of harm will arise from wrong ideas implanted in young minds. The ideal would be a public knowing the real fundamentals of physiology. This would generate the intellectual atmosphere in which quackery could not live. We are still a long way, however, from this consummation

[1] *Medical Science of To-day.* A Popular Account of the More Recent Developments in Medicine and Surgery. By Willmott Evans, M.D., B.S., B.Sc.Lond., F.R.C.S.Eng., Surgeon to the Royal Free Hospital, and Surgeon to the Hospital for Diseases of the Skin, Blackfriars. London: Seeley, Service, and Co., Ltd. 1912. (Demy 8vo, pp. 324; Illustrations 31. 5s. net.)

MESSRS. SIEMENS BROS. AND COMPANY have reprinted from the *Archives of the Roentgen Ray* the paper on diathermic treatment of disorders of the circulation, read by Professor Nagelschmidt of Berlin at the annual meeting of the British Medical Association at Birmingham last year. Copies can be obtained on application.

AN official report on the prevalence and distribution of rabies in the United States in the year 1911 is summarized in the *Medical Record* of August 24th. The most striking feature in the prevalence of the disease has been its spread to the Pacific Coast States, which apparently were entirely free from it at the time of a former investigation conducted by the Public Health and Marine Hospital Service in 1908. Thus, in California not a single case was reported in 1908, whereas 124 persons were treated for the disease in 1911. More cases were observed in New York than in any other single State. There 889 persons were treated and 13 deaths occurred. It is believed that there has been an increase in the prevalence of rabies throughout the States. In marked contrast with the increase among the lower animals is the diminution in human deaths, which amounted to only 12 per cent. of the total number of cases. This is attributed mainly to the opportunities of prompt treatment now available. In 1908 there were twenty-three institutions in the States where the Pasteur treatment could be carried out; now there are forty-two. In addition to these there are five firms and laboratories which supply material for inoculation to medical practitioners. In 1911 the Hygienic Laboratory in Washington distributed virus for 942 cases. In the year 1911 more than twice as many took the treatment as in 1908, when the whole number who are known to have availed themselves of this method of combating the affection was only about 1,500. In 1911, on the other hand, there were 4,625 persons who received the Pasteur treatment. The total number of deaths from rabies in the whole United States, so far as ascertained during 1911, was 98. Of these 44 occurred in children between the ages of 11 and 20.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE eighty-second meeting of the British Association which has just been held at Dundee and was attended by 2,379 persons, of whom between 1,200 and 1,300 were visitors. The presidential address by Professor Schäfer, which is printed elsewhere in the present issue, attracted a large audience. In moving a vote of thanks to the President for his address Lord Provost URQUHART announced a gift of £10,000 to the funds from Mr. J. K. Caird, a jute manufacturer of Dundee, who is a vice-president of the association.

The work of the meeting was distributed among twelve sections. Summaries of the proceedings of those having some relation to medicine are here given, mainly from the abstracts supplied by the officials of the association.

SECTION OF PHYSIOLOGY.

Physiological Basis of Memory and Abstraction.

Professor MAX VERWORN presented a communication on the physiological basis of memory and abstraction. He said that upon the basis of the neurone theory the problem which arose for physiological psychology was the ascertainment of the physiological conditions of the sensory processes in the neurones. Here one of the first problems was the question concerning the fundamental principle involved in *memory;* what were the traces remaining after excitation produced by the sensory processes in the neurones concerned? It had been assumed that they consisted in molecular alterations resulting from the process of excitation. The maintenance of that view met, however, with some difficulties. The molecular alterations brought about in the neurones by the stimulus during excitation consisted in a disturbance of the metabolic equilibrium by an increase of the processes of oxydative disintegration. Immediately following each excitation, however, metabolic equilibrium was again restored, and every trace of the excitation process seemed to be obliterated. Still, even though the process produced no molecular alteration, a secondary result remained, which consisted in an increase of the mass of the living cell. This only became clearly manifest when the excitation was of frequent recurrence, and was then expressed by functional hypertrophy. The presence of the latter could be demonstrated in the neurones as well as in every other living substance. It formed the cellular basis of memory by increasing the intensity of the discharge of impulses, and with this the specific action of the neurone. This recurring rsə led to a facilitation of the tracts (*Ausschleifen der Bahnen*), and thus to a greater intensity of the specific action of neurone chains resulting from the repeated demands. They found this demonstrated in the association processes of the cerebral cortex by the predominance of the facilitated conceptions, in response to a stimulus over others not practised in this manner. In lapse of memory resulting from want of practice, the reverse process took place. Here the unused neurones fell into an atrophy by inactivity. These facts afforded an insight into the process of *abstraction.* All their conceptions were complex in their nature. The abstract conceptions were distinguished from the concrete by the fact that the former, in the specific manner of the adjustment of their components, had no correlation in the sensory world, whereas the latter were fully in accordance with those things which could be perceived through the senses. In spite of this, however, the material from which the abstract conceptions were built up was likewise individually derived from sensory perception, and even the most abstract conceptions were composed of sensory elements. In the genesis of abstract conceptions the principle was merely that in a number of association complexes gained by sensory perception, having in part common, in part different components, the common components which returned with greater frequency were developed by use, whereas the special components, which recurred seldom, or not at all, fell into atrophy and receded into the background. As the neurone stations, which corresponded to the common components, underwent, by more frequent functional demands, a hypertrophy, they consequently predominated later in response to a stimulus by the intensity of their discharge, that was to say, of their specific function. Thus, finally a framework of associated conceptions was formed, in which only the most frequently recurring components of a complex were left over, whilst the other, through atrophy by inactivity, had disappeared more and more. There was to be found the reason that abstract conception in regard to their specific structure finally possessed no precise correlation in the world of sensory perception.

Relation of Mind and Body.

This question was discussed from the standpoint of philosophy by R. LATTA, M.A., D.Phil., from that of psychiatry by Sir THOMAS CLOUSTON, from that of physiology by Dr. J. S. HALDANE, and from that of psychology by H. J. WATT, Ph.D.

Mr. LATTA said there were two main groups of theories—the parallelist and the animist—the latter including various forms of the interaction theory. Parallelism consisted essentially in the hypothesis of a similar mechanical system of mental phenomena, corresponding point for point with the physical system, but entirely independent of it. They had thus in parallelism an *a priori* extension of the mechanical hypothesis to mind, although the basis of the mechanical hypothesis was the necessity of excluding everything mental from the physical system. Parallelism, accordingly, transformed the mechanical method from a scientific hypothesis into an ultimate metaphysical principle. On the other hand, the vitalist and animist theories insisted on the recognition of a teleological factor in organism and in mind. They were thus in direct opposition to the mechanical hypothesis, as it was originally conceived, for this hypothesis had as its aim the rejection of final causes as principles of explanation in the physical world. The mechanical view was right in rejecting final causes if they were regarded not as immanent in nature, but as external and accidental. The vitalist and the animist were right in insisting on a teleological factor; but they exposed themselves to criticism from the mechanist point of view by placing the teleological factor entirely outside the mechanical system. The two realms, the mechanical and the teleological, thus remained, on the animist view, as much apart as they were on the mechanical theory. But teleology was not external, but immanent. Wherever they had system, they had final cause, and the ends were not outside of the system. If the physical world was a real system, it had an immanent teleology. If the mental was also teleological, they might set aside the hypothesis of a gulf between the two realms, and recognize that the distinction of the physical from the psychical was a distinction within one system. This brought them nearer to the actual facts of experience, and it did not mean the merging of one system in the other or of both in a vague unity, for increasing knowledge meant more exact and real distinction, and at the same time the recognition of more profound unity between the things distinguished.

Sir THOMAS CLOUSTON said psychiatry implied the general assumption that body and mind had a constant and necessary relationship. The progress of physiological science and of modern psychology had compelled the psychiatrist to the conclusion that the brain was the necessary vehicle of mind, and therefore the study of brain in its physiological and pathological relations was by far the most important, indeed the absolutely necessary preliminary to his work. It was definitely proved that when the brain was not developed in a normal way from birth onwards the mental functions were also non-developed. Careful observation had also shown that under these conditions the outward expressions of mind, in the shape of speech and the workings of the mind muscles of expression in the face and eye, were usually accompanied by what had been wrongly termed the "stigmata of degeneration," which were really the stigmata of non-development. It was now almost demonstrated that all the serious forms of mental disease could be associated directly with microscopic and bio-chemical alterations in the brain cells. It was found that in the brain there was a general solidarity of action, which in a way might be said to correspond to the metaphysical doctrine of the unity of mind, but that doctrine had to be extended so as also to include the unity of mind and body. If the term "unity" went too far, the term "parallelism" was certainly a correct description of the facts. A bad nervous or mental heredity tended especially to interfere with the development process of body and mind. If that heredity was very strong they were apt to find

complete arrestment, as in idiocy; where it was less strong there were attacks of mania and a tendency to that mental arrestment and death which he had called "adolescent insanity" and subsequently "dementia," or mindlessness. The normal power of the brain and mind to react to stimuli was impaired in some way in all cases of mental disease. This abnormality necessarily upset the social life of the community. The highest quality required for human society was the power of mental inhibition, or self-control. They could not dissociate that from bodily inhibition. They concluded that there were centres of mental inhibition, probably situated in the fore part of the brain, which were non-developed in idiocy, poorly developed in the primitive races, and diseased in the insane. That was one of the most important relationships of body and mind, and one of the most essential parts of psychiatric study. He thought the facts which were often described by the terms "subconscious" and "subliminal consciousness" were much better understood and fell in with physiological facts by being looked on as merely molecular and bio-chemical changes within the brain cells.

Dr. J. S. HALDANE said that from the everyday point of view a man or higher animal was a personality consciously and purposively controlling, with greater or less success, his body, and through it the surrounding environment. From another point of view, however, the behaviour of the man was dependent on his physical environment and on the blind physical or physiological processes occurring around and within his body. His perceptions and actions and his continued existence depended absolutely on the material and energy coming to him from the environment, and on the bodily structure transmitted to him from his parents. From that point of view no independent mind or soul could be distinguished; all psychical activity was demonstrably dependent on the body and its physical environment, and was apparently a mere accompaniment of physiological changes. The facts on which that conclusion depended appeared at first sight to be unassailable, and to become more and more cogent with every year of advance in knowledge. Those two views clashed with each other. If they adopted the first view, they were faced by the hard facts on which the second view depended. If they adopted the second, they encountered the difficulty that, from their very nature, physical and mere physiological processes afforded no explanation of intelligent behaviour. The difficulties of the first view had been sufficiently emphasized in recent times in consequence of the great advances in physical and physiological knowledge. But the difficulties of the second view were equally great, if not greater. They had been admirably stated in Dr. McDougall's recent book on *Body and Mind*. These difficulties could not be solved by any theory of interaction between the body and the mind or soul. Between body and mind there was no interaction, simply because the body, more fully understood, was the mind. From the physical and chemical standpoint a man was about 70 kilog. of material with a certain configuration, properties, and internal movements — this material consisting of a great variety of chemical compounds, interacting upon one another in various ways. From the physiological standpoint the man was a living organism blindly fulfilling its biological destiny. From the psychological standpoint he was a person the subject of purposive knowledge and volition. The man as mere physical body or organism was an evident fiction or abstraction from reality, though a very necessary one for our imperfect knowledge. As a conscious individual personality he was at least far less of a fiction. But they could often get no further than a physical or a physiological account of reality, and for this reason the physical or physiological account of man could never be dispensed with. The physical sciences, biology and psychology, went on their several ways, accumulating knowledge which each science interpreted according to its own working hypothesis and subject to the limitations due to the abstractions from reality which were involved in these hypotheses. Each lower science also handed on what was, relatively speaking, raw material to the higher one. The attempt to resolve the higher into the lower, as by making mind dependent on body, was foredoomed to failure. The corresponding attempt to resolve the lower into the higher, as by making body dependent on mind, was also impossible, for the knowledge which would justify such an attempt was lacking, even were such knowledge conceivable. It seemed to him that it was only by this general conception of the relation of physical and physiological to psychological knowledge that each science could gain for itself a clear field of operation, and escape from the confusion which resulted from wrongly applying scientific hypothesis to what lay outside their scope. In practical life they drew no such distinctions; for they used every scrap of knowledge they possessed, however partial or mutually contradictory these scraps might be. They lived and moved in a world of apparent contradictions, within which science, religion, and philosophy afforded them only a general guidance. In this practical life the question of the relation of body to mind came up in a piecemeal form, and could only be answered in a similar fashion, as it was answered every day in practical medicine, where they sometimes regarded the body as dominating the mind, and sometimes the mind as dominating the body. Such answers were, however, not those of science or philosophy. They could not generalize from them, and the attempt to do so led only to confusion.

Dr. H. J. WATT, Ph.D., said the connexion between body and mind must rest upon some form of correlation between the realms of "material" and "psychical" things, in so far as it was agreed that some or all psychical processes were accompanied by, or are evoked by, physical processes or vice versa. The scope of this correlation could not well be explored from the physical side, as obviously not all physical processes and all aspects thereof had a discoverable psychical counterpart. The task for psychology was, then, to form an exhaustive list of psychical states, whether elements, compounds, or other derivatives, and to ask for each of these and each distinguishable aspect thereof whether some satisfactory correlative could not be found among known or possible psychical processes and their predicable aspects. Such a psychological analysis had been involved in every theory of the relation between body and mind. An attempt to reduce the scope of correlation by a proof that the unitary nature of psychical processes had no physical counterpart would be considered. Allowing for mind, not only the rule of the identity of indiscernibles, but also the integration of limited differences within partial identity, according to specific rules to be discovered, they need not invoke the soul or any other psychical agent in order to explain these rules, any more than in physical science they need assume the presence of "forces" to explain physical laws.

Professor STARLING thought the question had very little place in the physiological section. The distinction between mind and body had not been proved; how, then, was it possible to discuss any connexion between the two?

Professor LEONARD HILL said physiology got along best by leaving aside the question under discussion. Still, such questions had a lasting attraction for the human mind.

Sight Tests.

Dr. F. W. EDRIDGE-GREEN contributed a criticism of the report of the Departmental Committee on sight tests. This committee was appointed "to inquire what degree of colour-blindness or defective form-vision in persons holding responsible positions at sea causes them to be incompetent to discharge their duties; and to advise whether any, and if so what, alterations are desirable in the Board of Trade sight tests at present in force for persons serving or intending to serve in the merchant service or in fishing vessels, or in the way in which those tests are applied." He confined himself to the first part of the reference—namely, that dealing with colour-blindness. The committee recommended that the wool test should be retained, but further modifications made of it. They suggested that the skeins should be divided into groups and a dark brown skein added. The necessity of dealing with the whole of the skeins in doubtful cases was first pointed out by Dr. Edridge-Green over twenty years ago. The committee had missed the most important points in the use of any test in which wools were employed. The first was that the test should contain the confusion-colours of the colour-blind. The second that the four chief colour names, red, green, yellow, and blue, should be used. If the test did not contain these essentials normal-sighted persons

would be rejected and colour-blind persons passed. This he had demonstrated repeatedly. It was evident that the committee had not recognized how very defective a man might be and yet pass the official test of the Board of Trade with the ease and rapidity of a normal-sighted person. In the *Lancet* of June 22nd he had given an example of a very dangerous case of colour-blindness which he examined for the first time in the presence of Professors F. T. Trouton and A. W. Porter. Examined with the official test of the Board of Trade the examinee picked out and matched all five test-colours—green, rose, red, purple, and orange-yellow—easily and correctly; he did not touch a confusion-colour. Examined with the largest aperture of his lantern, measuring ¾ in. in diameter, at a distance of 20 ft. he called red A, nothing and green; yellow, red; red B, white; neutral, green; and green, white and red. The committee had recommended a lantern of their own construction. It was absolutely necessary to have means of regulating the luminosity of the light. In the lantern suggested by the committee this could not be done, and many colour-blind persons would learn the sequence and appearance of the colours, and with a little practice would tell all the colours on the method suggested by the committee. There were many other essential details which had not been considered, as, for instance, the exact spectroscopic composition of the colours. It would be seen from the report that the possibility of dispensing with it and going back to the wool test was mentioned. In fact, with the lantern suggested, it was quite possible that this might come about. The committee had recommended a classification of colour blindness based on the flicker method of photometry. The classification was based on two assumptions: First, that colour-blind persons could be classified by measuring the luminosity of their colour sensations; secondly, that it was possible to do this by the flicker method of photometry. There was the strongest evidence that neither assumption was justifiable. The classification was in direct opposition to the work of the ablest observers.

Pulmonary Gaseous Exchange in Apnoea.

Dr. T. H. MILROY read a paper on this subject. He said the apnoea produced in animals by forced pulmonary ventilation with air was evidently independent of the degree and rate of distension of the lungs so long as the total amount of air driven through the lungs remained the same. After freezing the vagi, apnoea could be produced in the same way as with the vagi intact. This period of apnoea could be markedly altered by replacing the air in the lungs at the close of the pumping by various gas mixtures. In all cases where gas mixtures rich in oxygen were employed in this way the duration of apnoea was markedly lengthened; in fact, the increase so produced was as great as if the ventilation throughout had been carried out with the mixture. On the other hand, gas mixtures which contained amounts of carbonic acid greater than that present in ordinary air produced shortening of the period of apnoea. As the carbonic acid in the gas mixture was raised, a point was at last reached when there was at first a loss in the carbonic acid present in the alveolar air, this point evidently indicating that percentage of carbonic acid which corresponded to a partial pressure of that gas above that present in the alveolar air at the beginning of apnoea. With gas mixtures which contained 10 per cent. or more carbonic acid breathing started very shortly after cessation of ventilation. When the air in the lungs after prolonged ventilation was replaced by nitrogen there passed out into the alveolar air both oxygen and carbonic acid. The oxygen which passed out under those conditions did not rise above 4 per cent., nor the carbonic acid above a slightly lower limit. In all cases where oxygen or air was left in the alveoli at the outset of apnoea the consumption of oxygen took place more rapidly than the excretion of carbonic acid, the respiratory quotient in the case where air was used being from 0.3 to 0.4. The passage out of carbonic acid from blood to alveolar gas mixture seemed to be mainly if not entirely dependent upon the difference of the pressure of that gas in blood and alveoli, and not upon the nature of the gas mixture in the alveoli. Thus carbonic acid passed out practically at the same rate from blood in lungs to alveolar gas mixtures, whether composed of nitrogen or oxygen or such other gas mixtures as were free from carbonic acid. Everything pointed to the gaseous exchange during the condition of apnoea being due to physical causes—namely, differences of pressure between the gases of the alveolar air and those in the pulmonary blood.

Nervous Induction in the Paths of the Pressure Sense.

In a communication on this subject Dr. MAX VON FREY said that if two sensitive points of the skin, so-called pressure spots, were irritated simultaneously, the subject might have the impression of a double stimulus or of a single one. In the former case three other effects could be observed, namely, a mutual influence of the irritations on their intensity, their clearness and location. These effects were bound to a certain proximity of the stimuli, and waned with their growing distance. There was, for instance, no effect from one hand upon the other, but a pronounced one from one finger upon the next or from a point on the forearm upon any one on the same segment. By comparing a stimulus H given on a finger, say, of the left hand, with another, V, following the first in two seconds on the right, it was possible by adjusting the intensity of the latter to make both equal. If now together with H a stimulus N were given on an adjoining finger of the left hand, the succeeding stimulus V appeared to the subject much too weak in comparison with H. To make V again equal with H, its intensity must be increased, according to the intensity of N, up to 80 per cent. of the original value. The subject moreover stated that the stimulus H, if accompanied by N, lost much of its clearness or distinctness, so that, in comparing it with V, it was difficult to arrive at a definite opinion as to its strength. According to the subject, the stimulus H acquired under these circumstances a dim, blunt, or blurred character. By repeating the irritation and directing the subject to concentrate his attention as far as possible upon H, the difficulty might be largely overcome. Finally, there was always a tendency of two simultaneous irritations to attract each other, that was to say, the subject was liable to underrate the distance between them, though he could judge it tolerably well if the two stimuli were given successively. The deception was so cogent that it was not overcome by the subject being acquainted with the real position of the irritated spots on the skin. If the stimuli were subjectively of equal strength, the dislocation of the corresponding sensations was the same for both, and they appeared therefore equally displaced towards the mid-point of the line which connected the spots on the skin. If the stimuli were of different intensity, the weaker was always drawn towards the stronger one. Dr. von Frey expressed the opinion that these mutual influences sufficiently explained the well-known difference between the successive and simultaneous space, threshold, or *Raumschwelle.*

Naturally and Artificially Aroused Impulses under the Influence of Nerve Blocks.

A communication on this subject was presented by CHARLES M. GRUBER (from the Physiological Laboratory of the University of Kansas). The research was begun by Dr. Hyde several years ago, at the suggestion of Dr. C. S. Sherrington, in his laboratory in the University of Liverpool, and was completed by one of Dr. Hyde's research students. It was conclusively proved by Gruber that impulses aroused by artificial stimuli in a frog's sciatic nerve could be suppressed by the tripolar electrical block, liquid air, and other blocking agencies, and that the tripolar block could be repeatedly introduced in the circuit without injury to the nerve's function; also that afferent impulses could be inhibited with weaker currents than could the efferent impulses. It was of interest, therefore, to ascertain whether natural impulses originated by higher centres in the brain were physically of the same nature as those electrically produced, and could be suppressed by the same kind and strength of block as could those artificially aroused by the electrical current. The experiments were conducted on the phrenic nerves in rabbits. By means of a modification of Head's method, records of the movements of the diaphragm and the thoracic wall were obtained. The artificial stimuli were the threshold strengths of the faradic currents applied to the phrenic nerve and its branches; the movements of the diaphragm in response to these stimuli and the contractions of the diaphragm

resulting from the stimuli emanating from higher centres in the brain were recorded and compared. If the phrenic nerve or any of its branches were stimulated with the weakest possible current, it was responded to, throughout the period, by a contraction of the diaphragm or an inspiratory phase of the side stimulated. The efficiency of the tripolar electrical block was compared with others, especially those produced by cocaine and freezing mixtures, and liquid air. It was found that the contractions of the diaphragm were suppressed the instant, and throughout the period, that the tripolar block circuit was closed. The strength of the block current was not more than $(2.8)^2$ volts, and this strength would suppress the cranial or naturally aroused impulses, as well as those originated by the very weakest faradic current. Only in one experiment did a block of $(1.4)^2$ volts inhibit the cranial, but not the artificially aroused, impulses. But cocaine, a freezing mixture of sodium chloride and ice, or liquid air, proved efficient in suppressing the impulses originated by the higher centres in the phrenic as well as those produced by electrical stimuli. But these blocks were inferior to the tripolar, because they could not be repeatedly employed without injury to the function of the nerve. The presence of afferent fibres in the phrenic was well demonstrated by a simple experiment. When the motor impulses on one side were blocked, and the phrenic stimulated centrally to the block, a change in rate and amplitude of the diaphragmatic movements of the opposite side followed, as was indicated by the contraction curves secured from the diaphragm slips on that side. The same result was obtained when the phrenic of one side was cut and the central cut end stimulated. Stimulating or irritating the peritoneum in different regions of the abdomen with intact phrenic was followed by a change in rate and force of the respiratory movements. This was, however, probably due to afferent stimuli from nerves other than the phrenic to the respiratory centre.

Pharmacological Action of Nitric Esters.

Dr. C. R. MARSHALL read a paper on the pharmacological action of nitric esters. He said that pharmacologically the nitric esters acted mainly on unstriped muscle fibre and especially on that of the arterioles. Their chief effect was to cause a fall of blood-pressure owing to dilatation of the blood vessels. That the action was peripheral might be shown by perfusion of the vessels of an isolated organ and in other ways. Doses producing distinct vaso-dilatation had no appreciable direct action on other organs of the body, but in men their administration was frequently followed by severe headache. The cause of this was unknown; but as it was not proportional to the degree of vaso-dilatation it could not, Dr. Marshall thought, be wholly ascribed to this action. Large doses caused methaemoglobinaemia, and if the dose were sufficiently large death, in mammals, resulted from this cause.

The nitric esters he had so far prepared and investigated were:

Methyl nitrate.	Dulcitol hexanitrate.
Ethyl nitrate.	Dulcitol pentanitrate.
n-Propyl nitrate.	Sorbitol hexanitrate.
iso-Propyl nitrate.	
n-Butyl nitrate.	
	Arabinose tetranitrate.
Glycol dinitrate.	Glucose pentanitrate.
	Fructose pentanitrate.
Glycerol trinitrate.	Rhamnose pentanitrate.
Glycerol dinitrate.	Quercite pentanitrate.
Methyl-glycerol dinitrate.	Saccharose octonitrate.
β-Dichlorhydrin nitrate.	
Erythritol tetranitrate.	Tartaric acid dinitrate.
Pentaerythritol tetranitrate.	Ethyl-tartaric acid dinitrate.
	Citric acid nitrate.
Arabitol pentanitrate.	Ethyl-citric acid nitrate.
Mannitol hexanitrate.	Nitric acid nitrate.
Mannitol pentanitrate.	Lactic acid nitrate.
Dimethyl-mannitol tetranitrate	Ethyl-lactic acid nitrate.
Tetramethyl-mannitol dinitrate.	Cellulose nitrates.
	Starch nitrate.

For the present purpose these substances might be divided into four groups:

(i) The nitric esters of the monhydric alcohols.
(ii) The nitric esters of the polyhydric alcohols and sugars.
(iii) The nitric esters of starch and cellulose.
(iv) The nitric esters of the acids and their alkyl compounds.

The members of the last two groups did not produce the characteristic vaso-dilatation of most nitric esters—the nitric esters of starch and cellulose owing to their insolubility in aqueous media, the nitric esters of the acids owing apparently to what might be termed the inhibiting influence of the carboxyl group. In the case of some of the ethyl esters of the last-named compounds a fall of blood pressure commenced some minutes after an intravenous injection, the cause of which had not yet been ascertained. The first group—the nitric esters of monhydric alcohols — were relatively weak vaso-dilators. Methyl nitrate, for example, injected intravenously, was more than ten times weaker than glycerol trinitrate, and when administered by the stomach to man, was more than a hundred times weaker. They could be given in sufficiently large doses to rabbits to produce unconsciousness without causing death. The vaso-dilating action of the remaining group—the nitric esters of the polyhydric alcohols and sugars—considering their solubility, was powerful. The differences exhibited by the various substances were mainly quantitative, and were largely due to the differences in solubility of the substances in aqueous media. The presence of hydroxyl groups, however, diminished the action of a substance as compared with the fully nitrated compound very considerably. The action of the nitric esters seemed to be associated with their susceptibility to reduction and conversion into nitrites under the influence of caustic alkalis, and it was not improbable that a similar reduction occurred in the muscle cells themselves.

The Ductless Glands.

The report of a committee, consisting of Professor Schäfer (Chairman), Professor Swale Vincent (Secretary), Professor A. B. Macallum, Dr. L. E. Shore, and Mrs. W. H. Thompson, was drawn up by the Secretary, stated that Mrs. Thompson had been continuing her investigations into the neck organs (thyroid, parathyroid, post-branchial body, carotid body, ventral branchial body, thymus, procoracoid, and propericardial bodies). Her attention had been chiefly directed to these various structures in the frog. It seemed to be fully established that in the case of the American frogs which came into their hands at Winnipeg the thymus disappeared or became invisible during the winter months. The Secretary could state definitely that the same was not true of winter frogs in Edinburgh. Drs. Gardner and Mothersill have continued their experiments upon the adrenals of the dog. They found, as others had done, that extirpation of one gland caused marked wasting of the animal, though this was of a temporary character. They were also able to confirm the statement of some writers that, after removal of one gland, there was a marked compensatory hypertrophy of the other. But the chief object of their experiments was to discover whether, when the animal survived, for some time with a minimum of adrenal substance, there was a notable hypertrophy of the chromophil tissues left behind, and in particular the abdominal chromophil body.[1] The experiments showed that such atrophy actually occurred. Professor Vincent had found that the abdominal chromophil body of the dog could be stained deep brown by means of potassium bichromate even when the animal was alive. This was a convenient way of extirpation of the body without surgical proceedings. But in some cases the brown colour had disappeared when the animal had been allowed to survive. The nature of the absorbable or soluble compound formed with the bichromate was being investigated. In conjunction with Mr. A. T. Cameron, Professor Vincent had commenced an investigation into the chemistry of the thyroid apparatus and the rôle of iodine in the economy. The work had not advanced far enough to allow any definite report to be made.

SECTION OF ANTHROPOLOGY.

Human Ancestry.

Professor G. ELLIOT SMITH, President of the Section, in his address said it was the advance in brain structure in far greater measure than anything else that determined the evolution of mammals, and it had been wholly responsible for their dominant position, their world-wide distribution, and the plasticity which had manifested itself in the marvellous variety of adaptations to every mode of

[1] See Vincent, *Proc. Roy. Soc.*, 1910.

life which the mammals had undergone. If they searched for the new feature in the brain which had made possible all their achievements it would be found in a cortical area to which eleven years ago he gave the name "neopallium." In the lowlier vertebrates each of the avenues of the senses led to a special part of the brain, and, although there were free communications between the regions allotted to the olfactory, visual, auditory, tactile, and other senses, there was no instrument for the adequate blending of impressions reaching the brain through these different portals, or for the storing of impressions, so as to awaken in consciousness the different properties of an object which appealed to several different senses. The lowlier vertebrates did not see, hear, or feel an object in the sense that we associated with those terms. The neopallium of the mammal provided a receptive organ for impressions of all the senses—touch, vision, and hearing, as well as taste and smell, among the rest—which enabled the effects of all such perceptions to manifest themselves unified in consciousness, and to become recorded in some way so that they could be revived again, each discrete sensory impression or the one idea excited by all of them, in associative memory. Moreover, it was the instrument by means of which all these perceptions, past and present, could be freely blended in consciousness, so that the animal was able to appreciate all the properties of any object to whatever sense they might appeal, and could benefit by past experience and so be educated. He maintained that the neopallium was a feature distinctive of the mammalian brain. In the forerunners of the mammalia the cerebral hemisphere was predominantly olfactory in function; and even when the true mammal emerged, and all the other senses received due representation in the neopallium, the animal's behaviour was still influenced to a much greater extent by smell impressions than by those of the other senses. The adoption of an arboreal life by some small insectivore-like creature, shortly before the dawn of the Tertiary period, and its subsequent cultivation of the sense of vision until it became a highly specialized anaptomorphid, enabled man's remotest primate ancestor to escape from the domination of the sense of smell as the guiding influence of its life, and to cultivate its other senses, so as immensely to widen the sensory avenues by which the outside world could affect its conscious activities. The arboreal life, which demanded great activity and agility, led to the special cultivation of skilled movements of the limbs, and such an acquirement was clearly facilitated in this group by the perfection of visual control, without which finely adjusted actions of the hands and feet could not easily be learnt. The acquisition of such skill in movement necessitated the increased perfection of the tactile and other sensory areas of the brain, so as the more nicely to control the adjustments and correlations of muscles essential for any precise action. Thus, they had a chain of linked influences that followed on the specialization of the visual apparatus in the brain of our primitive arboreal ancestor—the perfecting of touch and the acquirement of skill in action. The heightened acuity of vision and the expansion of the cortical area for storing visual impressions, together with the growing importance of touch, and in a less measure, perhaps, of hearing, immensely widened the psychical content of the life of the Eocene Tarsioid in comparison with that of its contemporaries; but there was yet another factor which its mode of life called into play which was fraught with the most far-reaching possibilities in the creation of man. The co-ordination of large groups of muscles for the purpose of performing some precise action, which must be controlled during the stage of learning by tactile, kinæsthetic, and visual impressions and memories—the fruits of experience—necessitated the formation of some cortical apparatus which would control and harmonize the activities of the various centres, regulating the muscular actions, and bringing the total sum of consciousness at any one moment to bear upon the performance of a given act. Out of such a necessity as this there sprang in the early ancestor of man and—though in much less degree, in certain other phyla also—an outgrowth of the motor cortex, which became the mechanism for attention and the orderly regulation of the psychical processes. Thus, at the very dawn of the Tertiary period, there were developed the germs of all the psychical greatness which, in the million or so of years that had followed, culminated in the human mind. The primates found in the branches of trees the asylum and protection necessary for the cultivation of brain and limbs during the period of their obscurity as an insignificant tribe, but, when they became powerful enough to hold their own and wax great, both in size and power, they had maintained sufficient of their primitive character and the plasticity that went with them to be able gradually to give up the arboreal mode of living, and to re-establish themselves once more as dwellers on the solid earth, competent to hold their own against all comers. He should be inclined to look upon the orang, the chimpanzee, and the gorilla, not as ancestral forms of man, but as the more unenterprising members of man's family, who were not able to maintain the high level of cerebral development of the feeble-bodied human, but saved themselves from extinction by the acquisition of great strength, and a certain degree of specialization of structure. The feebler man was able to overcome his enemies and maintain himself in the struggle for existence by his nimbleness of wit and his superior adaptability to varying circumstances. Man was the ultimate product of that line of ancestry which was never compelled to turn aside and adopt protective specializations either of structure or mode of life, which would be fatal to its plasticity and power of further development. Such advances as the assumption of the erect attitude were brought about simply because the brain had made skilled movements of the hands possible; yet once such a stage had been attained, the very act of liberating the hands for the performance of more delicate movements opened the way for a further advance in brain development to make the most of the more favourable conditions and the greater potentialities of the hands. If the earliest gibbons were already able to walk upright, how was it that they did not begin to use their hands for skilled work, and at once, before men? The obvious reason was that the brain had not yet attained a sufficiently high stage of development to suggest appropriate occupation for these competent hands to do, to the exclusion of their function in climbing. In the simian brain the sensory areas predominated, and the behaviour of the animal was to be looked upon as the response to the immediate sensory impressions of the moment; in the human brain the great association areas had grown far beyond the dimensions of the sensory areas, and experience, the effects of education, and knowledge assumed the dominant rôle in influencing conduct. It was easy to conceive how it came to happen, once the evolution of the brain made it possible for the ape to appreciate its ability to perform and anticipate the results of skilled action, that he at once began to avail himself of the larger life that was opened before him. The erect attitude became stereotyped and fixed and the limbs specialized, and these upright simians emerged from their ancestral forests in groups with the rudiments of all the powers that eventually enabled them to conquer the world. To the memories of the sounds of other animals and of the noises that occurred in nature, which had already become stored up in the sensorium of apes, the primitive ape-man added a collection of records of the expressive sounds deliberately emitted by his fellows, and in course of time the consciousness of these sounds was recorded, along with the memories of his gestures and grimaces, associating each with some meaning, which became a new way of communicating with his fellows. The perfection of the cortical mechanism for appreciating sounds and detecting a very wide range of qualities was associated in the human brain with a remarkable growth and differentiation of the auditory area of the cortex. As intercommunication between members of a social group became a matter of vital importance to the individuals composing it, this acuity in recognizing sounds of different pitch, tone, and timbre, and in detecting their precise emotional significance, would grow *pari passu* with the acquisition of speech. If it was the precocious high development of the sense of sight that started the primates on their career, the high development of the cortical mechanism for discriminating sounds played a great part in making man from an ape. He thought that most anthropologists who approached the study of speech from the physical or biological side had concentrated too much attention upon the supposed motor centres, and not enough on the great temporo-parietal areas, upon the education of which the faculty of speech

so largely depended. In other words, our simian ancestor must have had something to say before he attempted to find means of expressing it. They were apt to forget the immensity of the heritage that had come down to them from former generations of men, until they began dimly to realize that for the vast majority of mankind almost the sum total of their mental activities consisted of imitation or acquiring and using the common stock of knowledge. For this accumulation of knowledge and its transmission to our generation they were almost wholly indebted to the use of speech. When they considered how slowly and laboriously primitive man acquired new ideas, and how such ideas were treasured as priceless possessions and handed on from tribe to tribe, what became of the current theories of independent evolution of customs and culture? The modern problems of anthropology that they had to solve, those which related to man and his inventions, since the time of his world-wide distribution and differentiation into distinct races, were not so much questions of independent evolution, but rather those concerning the migrations, the intermixtures, and the blendings of different races and cultures. The hypothesis of the "fundamental similarity of the working of the human mind" was no more potent to explain the identity of customs in widely different parts of the world, the distribution of megalithic monuments, or the first appearance of metals in America, than it was to destroy their belief that one man, and one only, originally conceived the idea of the mechanical use to which steam could be applied, or that the electric battery was not independently evolved in each of the countries where it was now in use.

Ancient Egyptian Skulls.

Professor G. ELLIOT SMITH presented a communication on the physical characters of the human remains found by Mr. Quibell in mastabas of the Second and Third Dynasties at Sakkara. He said the problem that specially called for solution in the examination of this series of skulls was whether there was any evidence of foreign admixture in the population of the Second and Third Dynastic period in Lower Egypt, such as is known[2] to have occurred by the time of the Fifth Dynasty. The answer given by this material was quite definite. The people buried in these earliest Sakkara Mastabas showed numerous unmistakable alien traits; but at the same time they exhibited such a series of gradations, passing into the commoner type of Egyptian, as to raise for discussion the interesting problem whether real blending of characters occurs in human mixtures. The complete evidence and the discussion of its significance will be published in the report of the Committee of the Association on the Physical Characters of the Ancient Egyptians.

Early Mummification in Egypt.

Professor G. ELLIOT SMITH also read a paper on the earliest evidence of attempts at mummification in Egypt. He began by recalling that in previous notes[3] the earliest evidence of mummification in Egypt that he was prepared to admit as being unquestionable was that afforded by the mummy said to be that of Ra-nefer, found by Professor Flinders Petrie at Medûm in 1892, and now lodged in the Museum of the Royal College of Surgeons in London. The earliest date that could be assigned to this mummy was the age of Snefru, the beginning of the Fourth Dynasty, although he believed there were reasons for thinking it might belong to the period of the Fifth Dynasty. During a visit to Egypt last winter he was permitted by Mr. J. E. Quibell to examine the human remains found by him in a series of mastabas at Sakkara, belonging to the period of the end of the Second and the beginning of the Third Dynasties. In the burial chamber of one of these mastabas the skeleton of a woman about 35 years of age was found completely invested in a large series of bandages—more than sixteen layers still intact, and probably at least as many more destroyed—ten layers of fine bandage (warp seventeen and woof forty-eight threads to the centimetre), then six layers somewhat coarser cloth, and next to the body a series of badly corroded, very irregularly woven cloth, much coarser (warp six and woof fourteen per centimetre) than the intermediate and outer layers. Each leg was wrapped separately, and there was a large pad on the perineum. The bandages were broad sheets of linen rather than the usual narrow bandages. The body was flexed, as was usual at this period. In the wide interval between the bandages and the bones there was a large mass of extremely corroded linen, whereas the intermediate and superficial layers of cloth were quite well preserved and free from corrosion, except along a line where the cloth was corroded to represent the *rima pudendi*—a fact of great interest when it was recalled that in the Fifth and probably the Fourth Dynasties it was the custom to fashion (in the case of male mummies) an artificial phallus. The corrosion was presumptive evidence that some material (probably crude natron) was applied to the surface of the body with a view to its preservation. If so, this was the earliest body with unequivocal evidence of an attempt artificially to preserve or prevent decomposition in the soft tissues. The specimen is now in the Museum of the Royal College of Surgeons.

The Lesions Caused by Judicial Hanging.

Dr. FREDERIC WOOD-JONES presented a communication on this subject. He said that during the first season's field work of the Egyptian Government Survey of Nubia there was unearthed in the neighbourhood of Shellal a series of bodies, buried roughly in trenches, showing the effects of various forms of violent death. The physical type of these men—for all were adult males—indicated them to be intrusions on the typical Egyptian population. Their place of burial was within the walls of what had been a Roman frontier fort, and there was every indication that they had been executed in Roman times. One man actually had the hangman's rope *in situ* round his neck, and a very large number showed a curious lesion of the base of the skull. This lesion was diagnosed in the field as being caused by hanging, and its method of production seemed clear. However, when skulls of criminals were examined in museums it was found that this lesion did not exist in men known to have been "hanged." Quite recently (February, 1912) Dr. G. H. Edington had described the skull of Dr. Pritchard, unearthed during the demolition of old buildings in Jail Square, Glasgow. Pritchard was executed in 1865, and the actual circumstances of his hanging were known. The interest of the case lay in the fact that Pritchard's skull showed the same lesions as were displayed by the men executed by the Romans in Nubia. Methods of hanging had changed from time to time, and the lesions produced had been studied by many people; but there was still a great want of agreement in the ideas as to the actual injury inflicted. It was probable that in the cases of Pritchard and the Nubian men much the same methods were employed; but in the history of English judicial hanging the variation in method had easily accounted for the variety of lesions which had been found and claimed as the cause of death. The reason why no lesion was found in so many museum specimens was probably to be sought in this evolution of hanging. "Hanging" might imply (1) the hanging of a corpse; (2) the hanging (strangulation) of a living being; (3) or the dropping and hanging used to-day as the form of judicial death in England. Each had its historical aspect and its anthropological and pathological interest.

SECTION OF EDUCATIONAL SCIENCE.

School Books and Eyesight.

The report of the committee which inquired into the influence of school books upon eyesight, and was presided over by Dr. G. A. Auden, was presented.

The committee stated that the replies which it had received to its inquiries showed that it was not customy for teachers to give the children special instruction concerning the care of their eyes. It was stated in several instances that teaching of that kind was given incidentally in the course of the lessons on hygiene which formed part of the school curriculum; but nothing more was done, and what was done amounted to very little. While effective arrangements for the detection of existing defects in the eyesight of elementary school children were general, and arrangements for the supply of proper spectacles at cheap rates were not uncommon, practically no systematic attention was given to the influence of school books upon eyesight. In the opinion of the committee, the existence of a

[2] *Ancient Egyptians* (Harpers), London, 1911, p. 114.
[3] Notes on Mummies, *Cairo Scientific Journal*, February, 1908; *Nature*, 78, p. 342; The History of Mummification, *Proc. Roy. Phil. Soc. of Glasgow*, 1910.

very serious amount of visual defect among children of school age was established as a result of official inspection. Some portion of that defect was preventable by greater care in the selection of books. It was desirable that a standard of book production should be established, and that the publication of books below standard should cease. It appeared possible that the adoption by local education authorities of a common standard would render unprofitable the publication of books which failed to reach such standard. It was hoped that the report might assist the responsible authorities in the work of determining the standard of book production requisite for the protection of the eyesight of children so far as it was influenced by the books which the children were compelled to read in school.

LITERARY NOTES.

MESSRS. D. APPLETON AND CO. will publish on September 18th a new edition (the eighth) of the *Principles and Practice of Medicine*, by Sir William Osler. This new edition has been entirely rearranged and rewritten. All the sections have been revised in the light of recent advances in medicine, and many new sections have been added. This well-known textbook is such a tried favourite with students and practitioners that many will be glad to learn of the appearance of this new edition.

Mr. H. K. Lewis (Gower Street) writes under date August 30th:

I notice in to-day's issue there is a review of the late Sir Henry Butlin's three lectures on the parasite of cancer. I should be greatly obliged if you could see your way to inserting a notice in your next issue mentioning that the book in question was printed (to Lady Butlin's order) *for private circulation only*, and that it is not therefore "*published*" nor obtainable from us (the imprint on the title page says "Printed by"). The whole edition was delivered to Lady Butlin as soon as it came from the binders. With regard to the appearance of the volume, your reviewer is of course entitled to his opinion, but I believe that both Lady Butlin and the Editor were satisfied with the appearance of the book.

Mr. L. L. Shadwell has edited *Enactments in Parliament Specially Concerning the Universities of Oxford and Cambridge*. The work, which is in four volumes, is published by Mr. Henry Frowde. The following interesting extracts relating to quacks is given in *The Periodical:*

Forasmoche as the science and connyng of Physyke and Surgerie to the perfecte knowlege wherof bee requisite bothe grete lernyng and ripe experience ys daily within this Royalme excercised by a grete multitude of ignoraunt persones of whom the grete partie have no maner of insight in the same nor in any other kynde of lernyng some also can no lettres on the boke soofarfurth that common Artificers as Smythes Wevers and Women boldely and custumably take upon theim grete curis and thyngys of great difficultie In the which they partely use socery and which crafte partely applie such medicyne unto the disease as be verey noyous and nothyng metely therfore to the high displeasoure of God great infamye to the faculties and the grevous hurte damage and distruccion of many of the Kynges liege people most specially of them that cannot descerne the uncunnyng from the cunnyng; Be it therfore to the suertie and comfort of all maner people by the auctoritie of thys present parliament enacted that noo person within the Citie of London nor within vij myles of the same take upon hym to excercise and occupie as a Phisicion or Surgion except he be first examined approved and admitted by the Bisshopp of London or by the Dean of Poules for the tyme beyng callyng to hym or them iiij Doctours of Phisyk and for Surgerie other expert persones in that facultie And for the first examynacion such as they shall thynk convenient; And afterward alway iiij of them that have been soo approved upon the payn of forfeytour for every moneth that they doo occupie as Phisicions or Surgeons not admitted nor examined after the tenour of thys Acte of v li to be employed the oon half therof to thuse of our Soveraign Lord the Kyng and the other half therof to any person that wyll sue for it by accion of dette in which no Wageour of Lawe nor proteccion shalbe allowed. . . .—3° Henry VIII, ch. xi (1511-12).

And where that in Diocesys of Englond oute of London it is not light to fynde alwey Men hable to sufficiauntly examyn after the Statute such as shalbe admytted to excersyse Physyk in them, that it may be enacted in this present parliament, that noo person fromhensforth be suffred to excercyse or practyse in Physyk through Englond untill such tyme that he be examined at London by the said President and three of the said Electys; And to have frome the said President or Electys lettres testimonialx of their approvyng and examinacion, except he be a Graduat of Oxforde or Cantebrygge which hath accomplisshed all thing for his fourme without any grace. . . .—14° and 15° Henry VIII, ch. v (1523).

With reference to this passage Mr. Shadwell notes that "although a graduate in medicine of Oxford or Cambridge who has complied with the requirements of this section is entitled under this Act and 3 Hen. 8, c. 11, to practise medicine or surgery with incurring penalties, there are now many privileges, including the right to recover at law his charges for services and medicines supplied, which he can only enjoy if registered under the Medical Acts."

In his *Physiology of the London Medical Student*, Albert Smith tells of an acquaintance who "when he was grinding for the Hall," being naturally of a melodious and harmonic disposition, conceived the idea of learning the whole of the practice of physic by setting a description of the diseases to music. He had a song of some hundred and twenty verses, which he called the poetry of *Steggall's Manual*, and this he put to the tune of "The good old days of Adam and Eve." Smith gives as examples the verses relating to the symptoms, treatment, and causes of haemoptysis and haematemesis:

A sensation of weight and oppression at the chest, sirs;
With tickling at the larynx, which scarcely gives you rest, sirs;
Full hard pulse, salt taste, and tongue very white, sirs;
And blood brought up in coughing of colour very bright, sirs;
It depends on causes three—the first's exhalation;
The next a ruptured artery—the third, ulceration.
In treatment we may bleed, keep the patient cool and quiet,
Acid drinks, digitalis, and attend to a mild diet.
Sing hey, sing ho, we do not grieve
When this formidable illness takes its leave.

Clotted blood is thrown up, in colour very black, sirs,
And generally sudden, as it comes up in a crack, sirs.
It's preceded at the stomach by a weighty sensation;
But nothing appears ruptured upon examination.
It differs from the last, by the particles thrown off, sirs,
Being denser, deeper-coloured, and without a bit of cough, sirs.
In plethoric habits bleed, and some acid draughts pour in, gents,
With Oleum terebinthinae (small doses) and astringents.
Sing hey, sing ho; if you think the lesion spacious,
The acetate of lead is found very efficacious.

Thus, says Smith, in a few lines a great deal of valuable professional information is conveyed, at the same time that the tedium of much study is relieved by the harmony. This medical poet appears to have been taken as a model by Mr. A. Ager, who describes himself as Medical Dispenser and "Author of *Magnus Stibium—The Alchemist*, *The Poetry of Bones*, and many other interesting Poems." Mr. Ager has been good enough to send us a few specimens of these poems. One of them, entitled the *Geography of Medicine*, begins:

Plants indigenous to Britain
Are well worthy to maintain
And better pay to cultivate
Than have them form (? from) a Foreign State,
The needs of Medicine should be made
Of more importance than sharp trade.
England supplies the useful Dill,
But Anise comes from Malta still.
India's a fertile region for Drugs
Exported in bags, boxes, or kegs;
Chiretta, Hemidesmus,
And Kamala come hence to us,
Galbanum, and Fruit Bael
Are sent over here as well;
Popular Caster (*sic*) Oil Plant;
Has leaves quite luxuriant;
Trimivelly provides Senna,
The Punjaub, Assafoetida,
Catechu, from the gulph of Kutch,
In Diarrhea is used much,
Bengal sends over Indigo,
Malabar, Cardamous (*sic*) and Kino;
Of Oil Cajeput a good store
Comes from the port of Singapore;
And the drastic purge Croton
Grows chiefly in the Isle Ceylon.

A curious piece of information is contained in the following verse:

Quassia is a bitter Plant,
Name derived from Negro servant;
Who gave it in doses of half a drachm,
For Fevers endemic at Surinam.

On the whole, we think it would be easier to learn materia medica in the ordinary way than from lines so halting as Mr. Ager's.

British Medical Journal.

SATURDAY, SEPTEMBER 14TH, 1912.

THE PRESENT STATE OF ANAESTHESIA.

The Section of Anaesthetics has done notable work in presenting to the members of the Association some of the more recent developments in the department of anaesthesia. Although there is no novelty in the actual method of intravenous infusion of drugs, yet it is only since the experiments of Burghardt, Kummel, and others proved the comparative safety of inducing general anaesthesia by such a plan that serious attention has been directed to what promises to be a beneficial advance in this department of surgical technique.

Mr. Rood's paper, giving the results obtained by him and his colleagues at University College Hospital, affords solid evidence in favour of intravenous ether infusion. The apparatus, which is due to him, is an advance upon previous attempts, and the continuous as opposed to the intermittent flow of infusing liquid obviates one of the most serious dangers of the earlier workers—clotting in the cannula. We are warned, both by the President in his introductory speech and by those who took part in the discussion, not to expect that the intravenous method will remove all the dangers and difficulties of anaesthesia. Its use, we are told, requires a discriminating choice of cases, as well as a mastery of its somewhat exacting technique. But this is no detriment at the present time, when the administration of anaesthetics has earned an independent position in surgery, and when those who have established their claim to a mastery of both the medical and the surgical side of the speciality are prepared for, and indeed desire, full responsibility in regard to the patient as he passes through the state of narcosis. There are many conditions—such, for example, as those which engender post-operative acidosis and its allied states, general septic invasion, the various pathological lesions which contraindicate the use of chloroform or ether by routine methods—in which the intravenous method has proved of singular value. The after-effects, it is claimed, are few and transient, and provided the details of the methods are carried out in their entirety, its dangers are only those which a judicious and skilled anaesthetist can foresee and discount. From the surgeon's point of view, again, the plan offers material advantages, and since it is the surgeon's manipulations which are involved in the ultimate success of the operation, a double advantage accrues to the patient. The larger question, whether other drugs are preferable to ether for intravenous infusion, cannot as yet be dealt with conclusively, since the evidence is too conflicting. The case of hedonal in this connexion has been recently considered in our columns. It is true that Mr. Leedham-Green, in opening the discussion upon the relative value of the methods of inducing anaesthesia and analgesia, deprecated intravenous ether infusion, but every novel method must pass through a period of careful trial before all workers accept it unreservedly. We have recently been reminded that that great scientist, Dr. Dionysius Lardner, scouted as an absurd impossibility the suggestion that ships could by steam power ever cross the Atlantic. *Quot homines, tot sententiae.* It is the thoughtful doubter who eventually increases the world's commonwealth of knowledge.

Yet a further development of the methods of inducing anaesthesia by ether was shown and discussed at the Section. The intratracheal method introduced by Meltzer and Auer was brought before the meeting by Dr. Ehrenfried, of Boston, and Mr. R. E. Kelly, of Liverpool, both of whom have modified in different ways the apparatus due to Elsberg. In spite of a somewhat complicated technique involved in intubating the larynx and the less simple character of the apparatus required, the intratracheal method has won for itself wide popularity and obviously possesses incontrovertible merits. Even if its adoption in routine practice is at present doubtful, its advantages are obvious for operations upon the lungs when a *plus* pressure in the pulmonary cavities is desirable. Both of these methods, however, owe in no small measure their freedom from serious pulmonary after-effects to the prior use of alkaloidal bodies, such as atropine, morphine, and scopolamine.

It was amply demonstrated at one of the sectional discussions that the preliminary injection of atropine abrogates hypersecretion of mucus and of saliva; while the true narcotics scopolamine and morphine subserve other useful purposes. As was pointed out by the President, who has made a special study of these drugs, there seems to be little doubt that the best results are obtained when these alkaloids are used in combination in appropriate proportions. The valuable work of the President of the British Association upon the protective value of atropine against vagal inhibition affords an additional reason for its use in all cases in which grave surgical shock is likely to arise. Professor Schäfer also, in his early work on the adrenal bodies, was led to anticipate great assistance from the adrenal extracts in cases of anaesthetic collapse. Upon this subject Dr. Goodman Levy's illuminating paper, read before the Section, issued a most important caveat against the indiscriminate employment of adrenalin in chloroformed subjects. Dr. Levy has found that during incomplete anaesthesia under chloroform the injection of even a small quantity of adrenalin causes the heart to pass into a condition characterized by a rapid succession of small beats; sometimes these are equal, but sometimes are irregular, being interspersed with intervals of arrhythmia. Electro-cardiographic tracings demonstrate that the normal procession of events in the systole is completely deranged, a multiple extrasystolic ventricular tachycardia supervening. Ultimately ventricular fibrillation arises, and, in the case of the lower animals, death ensues. That this is not a danger occurring in physiological research alone, seems evident from the cited cases of human beings who have collapsed, some fatally, when given adrenalin during incomplete anaesthesia. The research seems also to offer a probable explanation of many sudden deaths which have been attributed in the past to chloroform, cocaine, or fear, while the probable cause —adrenalin—has been overlooked.

The report of the work of the Section which appears in the present issue is well worth both reading and careful study, alike from the side of practical anaesthetics and from that of the *quaestio vexata* the professional status of the anaesthetist.

THE COLOUR OF FLOWERS AND HEREDITY.

DUNDEE can congratulate itself on the fact that this year's meeting of the British Association has marked —in the Chemical Section at any rate—a very definite stage of advance in the history of botany. The paper by Dr. E. F. Armstrong and Professor Keeble on the coloration of flowers vindicates the claim of the chemist to stand on an equal footing with the biologist as a student of heredity. Of the interest of the paper there can be no doubt, and, unfortunately, of its difficultiy one can speak with equal certainty, for on the complexities of Mendelism it superimposes a good deal of chemistry that is by no means clear even to the trained chemist. The main result achieved can be stated simply. The authors have given experimental proof of the brilliant hypothesis conceived by Bateson, that three factors are necessary for the production of colour in a flower —the hidden factor for colour, the factor that enables the colour to show itself, and, lastly, the absence of an inhibitory factor, which might almost be regarded as a double negative concept. The paper concerns itself with all three factors; but at last definite proof has been supplied of the existence of this inhibitory factor. So much for the fact established, but in their method the authors have gone a great deal further, for, instead of studying the mere outward manifestation of such phenomena as colour, square stems, wrinkled or smooth seeds, and so forth, they have pushed the research a stage further back, and have directed their gaze on the chemical substances through which these outward manifestations are produced.

Their paper can best be approached from the standpoint of a Mendelian problem. It is many years now since Professor Bateson "discovered" Mendel and began with the school of Cambridge biologists to extend the scope of his experiments. At first all was plain sailing, but one of the difficulties he had later to contend with was that on crossing white with coloured flowers he got in the second generation two results: the offspring appeared either in the ratio of three plants with coloured flowers and one with a white flower, or three plants with white flowers and one with a coloured flower. To explain this apparent anomaly he hit upon the hypothesis that many white flowers were really coloured, but that they contained, in addition to the factors that produced colour, a third factor that prevented its making its appearance. The idea was a working hypothesis, justified in so far as it helped to group and correlate a number of known facts but based on no sound foundation. To get a stage further it was clearly necessary to study the factors themselves. As the result of the work of Dr. Armstrong, Professor Keeble, and a number of other workers, the mechanism of flower coloration has been established.

Colour appears in a plant as the result of the union between a colourless substance known as chromogen and an oxidizing agent known as oxidase. One can fairly imagine the grains of chromogen lying in the cells of a flower petal starving for want of oxygen, just as the cells of the red-blooded animal are dependent for their oxygen on the circulating blood. The oxidase in its relation to the chromogen in the flower petal corresponds with the oxyhaemoglobin in the blood. When it has not its load of oxygen it is powerless to help the chromogen, and it is known as a peroxidase. The first stage in the inquiry was to try and mimic the process that goes on naturally in the flower. Most white flowers are without chromogen, and in the flowers with which they experimented the authors were able to make sure, thanks to the work of the Bateson school, that they were dealing with such flowers. They supplied their chromogen artificially, making use of some of the colourless coal-tar products, and soaking the petals in a weak solution of them made up with alcohol. Naturally, nothing occurred. The cells of the petal contained only peroxidase—that is, oxidase without the oxygen—necessary for it to perform its function. Oxygen was then added in the form of peroxide of hydrogen, and the whole cycle of petal coloration was set agoing. The peroxidase got the oxygen it required and turned to oxidase, the coal-tar substances that were masquerading as chromogen got their oxygen from the oxidase, and the white petals, in obedience to the same chemical laws that have governed the appearance of colours in flowers ever since the flowering plants appeared on the earth, blushed into colour whenever the oxidase was to be found in their cell substance. But the giving of oxygen to the peroxidase is not the first step of this house-that-Jack-built sequence. The chromogen grains are as a matter of fact tightly locked up with the sugar in the plant. Just as Professor Schäfer reminded us that the various glands of the body are continually sending out their hormones or chemical messengers instructing other cells to burst into active work, so the scent of the flowers is, so far as we know, the first stage in the long sequence of events. As the perfume of the plant spreads throughout the system, it unties the bonds that link the sugar and the chromogen, the peroxidase picks up its oxygen, and, according to the type of chromogen present, the blooms appear in all their several glories. The place of the scent can be taken by chloroform, alcohol, and carbonic and prussic acids.

Death has been defined by more than one physiologist as the completing of the act of life, and the changes just described are the changes that occur on the threshold of the death-chamber. Ordinarily the plant cell is in equilibrium, but the aroma of the flower is the signal that heralds the end. The water in the cells, concentrated in multiple molecules, breaks down into single molecules; the cell thus becomes swollen, and the down-grade changes set in until death comes at the end. There is a profound truth in the analogy between death and night. It is in the hours of darkness that the plant builds up the oxidase that is to call out the signal that the flower has run its full course, that is to announce that the fruit has ripened, and that the end of its activities has been reached. The blackening of the ripe banana, the browning of the cut apple, are actuated by the same mechanism — the union of the oxidase and the chromogen.

And now of the inhibitory factor that masks the latent colour in many of the white flowers. Its existence could be proved if a way were devised to remove it and the latent colours were then to reappear. This has not yet been achieved, but the proof of the contention advanced is cogent. The difficulty has been to show that such flowers possessed oxidase and that it was the inhibitory factor that prevented its operation. The authors have succeeded in removing the inhibitory factor by prussic acid, and they have then found that the colour from their coal-tar chromogen appeared just as theory demanded.

This experiment and the others by which the general theory has been worked out is a triumph for the bio-chemists, and justifies the many efforts which for

the last few years they have been making to carry chemistry into what is almost a new sphere of activity. The achievement is important in its actual results, and still more so in the future developments which it foreshadows. As Professor Keeble pointed out in his address to the botanists, there is much that is obscure in the regular cycle of activities that the plants alternate through by day and night. These, there can be no doubt, have their chemical counterpart, and, there can be little doubt, have their chemical cause. The paper that these investigators read is the public avowal of the chemists that the question is one that they intend to investigate, and the success already achieved is of good augury for the future. It is also a striking instance of the need for increased co-operation between the exponents of the different departments of science.

PATHOLOGY AND TREATMENT OF ECLAMPSIA.

DURING the past few years the treatment of eclampsia has undergone radical changes owing to investigations which have lately thrown much light on the pathology of the condition. Kermauner has recently published a critical review of the subject.[1] He points out that, owing to its frequent association with degenerative changes in the kidneys, eclampsia was formerly attributed to the retention in the blood of albuminous matter or of chlorides. But, as Zangemeister has shown, the symptoms may pass off before the functional activity of the kidneys is restored; and, moreover, diuretics are of little use in the treatment of the attacks.

More recently two theories have been advanced. One is that eclampsia is an anaphylactic phenomenon. This has been practically disproved by experiments on animals, which show that injection of fetal serum causes absolutely no symptoms. The other is that some substance of the nature of a ferment, accumulating in the maternal blood, leads to an outbreak of eclamptic symptoms. The placenta has hitherto been thought to be the source of this ferment, but Dienst has put forward a theory of its origin which goes far to explain the various pathological conditions now known to be associated with eclampsia. These conditions are: (1) The so-called "liver of pregnancy"—a pathological condition characterized by congestive changes in the lobules of the liver, which are not quite the same in primiparae as in multiparae. In the former the venules in the periphery of the lobule are congested; in the latter there is a block in the central vein itself. Dienst attributes this condition in primiparae to the increased abdominal pressure reacting on the portal vein, but in multiparae to cardiac insufficiency. (2) Multiple thromboses and necrotic areas in the liver, which have been observed in all cases examined after death. (3) Changes in the kidneys, the so-called nephrosis gravidarum, which are pathological. (4) Thrombosis, and perhaps necrosis, in various organs.

Dienst discerns a relationship between these several lesions, and traces all the symptoms to an excess of fibrinogen and fibrin ferment in the blood during pregnancy. Experiments on animals prove that both these substances, even in small quantities, are capable of causing convulsions and death. Fibrinogen is normally present in blood to the extent of 0.3 per cent.; it is increased during pregnancy, and in eclampsia reaches 0.6 per cent. or more. It is produced by the destruction of leucocytes, and is destroyed or eliminated in the lungs. Fibrin ferment, on the other hand, is not a normal constituent of the blood. From the red corpuscles, however, is formed "thrombogen," a substance which is neutralized at once by a product of the liver cells known as "antithrombin." If the formation of this is hindered by degenerative changes in the liver, the thrombogen is left free to unite with "thrombokinase" (also formed by the destruction of blood corpuscles) to form "prothrombin," and this latter substance is converted, in the presence of calcium salts, into fibrin ferment. During pregnancy, when large numbers of blood cells are being used up in the placenta for the nourishment of the embryo, a large proportion of the fibrin-forming elements is present in that region, and the coagulability of the placental blood is thereby raised. The venous blood of the uterus is comparatively poor in calcium, but this necessary factor of coagulation is supplied when it becomes mixed in the right heart with the blood from the portal vein. If this portal blood be deficient in antithrombin, fibrin ferment is formed and fibrin is precipitated. The liver, as stated above, may be congested during pregnancy, and Dienst points out that this would lead to a deficiency of antithrombin, and so favour thrombosis in various distant organs. At the same time the excess of fibrin ferment produced in the blood acts as a toxin and causes the eclamptic convulsions. The lesions in the kidneys only contribute to eclampsia by impairing excretion and thus hindering the elimination of the toxic fibrin ferment. Cases in which the outbreak of convulsions is sudden and in which no lesions can subsequently be discovered remain to be explained. Dienst suggests that, owing to the stasis produced in the veins of the lower half of the body by the pressure of the gravid uterus, an excess of prothrombin may be accumulated in them. When this pressure is removed by the emptying of the uterus at birth, the congested vessels are relieved and the general circulation suddenly becomes flooded with the fibrin ferment elements which they contained. Owing to the lack of a corresponding increase of antithrombin, fibrin is formed and toxic symptoms are produced.

To explain cases of eclampsia arising some days after delivery, Dienst points out that with the birth of the child the destruction of the corpuscles in the mother's blood ceases, and the products of degeneration are eliminated either in the lochia or by the kidneys, or even through the process of lactation. But when excretion is inhibited by serious lesions in the kidneys or by suppression of the lochia, then the renewed destruction of leucocytes which accompanies the formation of milk, by giving rise to the production of more fibrinogen, may determine an attack of eclampsia during the puerperium.

The theory of Dienst has a direct bearing on the treatment of eclampsia. If it be correct, then it would seem that the sooner the pregnancy was terminated the better. The increase in leucocytosis is thereby arrested, and the breaking up of the numerous leucocytes into toxic substances ceases. But the author does not favour rapid delivery, both on general grounds and on account of the special risk already mentioned of flooding the circulation with toxic elements by the sudden relief of pressure on the veins. Diuretics, he considers, have only a restricted value, they do not check the formation of toxins, and can only hasten their elimination if the kidneys are sound. He recommends venesection even after delivery, and thinks that curetting of the uterus, not only to remove clots, etc., but also to aid the elimination of poisons by the lochial flow, is useful in cases arising

[1] *Wien. med. Woch.*, No. 23, 1912.

during the puerperium. He considers that operations such as amputation of the breasts and decapsulation of the kidneys do good only through the loss of blood which they cause. As a preventive measure the administration of lemon juice is said to be beneficial. It stimulates the liver to increased formation of antithrombin (Nolf) and diminishes the coagulability of the blood. With a view to preventing congestion in the hepatic lobules, Dienst thinks it advisable in multiparae to give digitalis to strengthen the heart's action, while in primiparae anything which tends to lessen the intra-abdominal pressure—such as purgation, washing out the stomach, or even the spontaneous vomiting of pregnancy—must be regarded as beneficial. The good results which have followed the administration of chloroform and chloral hydrate may, he thinks, be in part due to their dilating the renal vessels and so favouring the excretion of toxins. But as all narcotics, especially those allied to morphine, tend ultimately to produce degeneration of glandular organs, he considers that further investigation as to their effects on the structure and functions of cells is necessary.

"DOCTORS' BOYCOTT" AND POLITICIANS' BLUFF.

UNDER the heading "Failure of the Doctors' Boycott Anticipated" some amazing statements are made by the London correspondent of the *Liverpool Post* and the *Liverpool Echo*. This oracle announces that the Insurance Commissioners have now abundant and clear evidence that they will be in a position to work the medical clauses of the Insurance Act without the co-operation of the British Medical Association. The confiding public is assured that the proposed boycott of the Act is now practically a failure. There has, it is said, been a striking addition to the ranks of the medical men anxious and willing to give the Act a fair trial, and among them, it is added, there are a number of distinguished consultants and "members of what is generally described as the Harley Street school." This phrase is of itself sufficient to show how intimately the inspired correspondent is acquainted with medical matters. He goes on to say that "Mr. Lloyd George is, of course, entirely acquainted with the position which has now arisen—a fact which no doubt explains the equanimity with which he has for some little time regarded the situation." No one has denied the Chancellor the attribute of courage, and doubtless even if his Act should tumble about his ears, *impavidum ferient ruinae*. But to the ordinary person who has not been admitted behind the shrine, it may appear that the correspondent who has been chosen as the medium of this weighty revelation doth protest too much. In the style of the message there rings the note of bluff with which we have become so familiar that it has lost any effect it might once have had. If the Commissioners cherish the belief attributed to them, the "abundant and clear evidence" which they profess to have is a possession peculiar to themselves. The British Medical Association has no evidence which lends a shadow of support to the statement, and the Medical Secretary has taken the earliest opportunity of stating publicly that the Association has no reason to believe that the vast majority of the profession has in any way changed its attitude towards the Act. In so numerous a body it is inconceivable that all its members should be unanimous as to a complicated and far-reaching question such as State Insurance. But we may state confidently that, notwithstanding a few backslidings and defections here and there, the mass of the profession stands firm in its determination to hold aloof from the working of the Act until its reasonable demands are conceded. The insidious attempts that have been made to sow dissension in our ranks have so far signally failed. This latest manœuvre will, we are sure, prove equally futile.

MR. LOCH ON NATIONAL HEALTH INSURANCE.

THE Charity Organization Society has issued the twenty-first edition of that valuable work the *Annual Charities Register and Digest*,[1] with an exceedingly useful introduction by Mr. Loch on "How to Help Cases of Distress." In the discussion of this pressing question he sets down the principles which should underlie true charity, laying special emphasis on the necessity of helping needy men and women to help themselves. "Charity," he says, "for those who are in distress has to open the way to sober living, health, and self-support"; and again, "Charity which seduces the individual from the wise and natural toilsomeness of life, and which does not push him forward to self-maintenance, is the poor man's greatest foe." After reading these statements one turns with interest to a new section in the introduction dealing with National Health Insurance, in the hope that Mr. Loch, out of his full knowledge, will lighten the gloom which still hangs over the National Insurance Act. This expectation is not fulfilled. The writer contents himself with giving us a synopsis of the Act, passing in lucid review the main sections grouped under the following headings: "Who are Compelled to Contribute?" "What are the Contributions?" "What is the Relation to Friendly Societies?" "What are Insurance Committees and their Duties?" "What is the Relation of the Scheme to Hospitals," etc.? Mr. Loch seems to err with many others in thinking that employees who have more than £160 per annum are excluded from the compulsory operation of the Act, forgetting that for the manual labourer there is no wage limit. He corrects this lapse, however, by saying that "the chief exclusion is that of employment with too high a remuneration and otherwise than by manual labour.' When considering the duties of the Insurance Committees he draws attention to Section 63, which deals with excessive sickness among insured persons as the result of insanitary conditions. The successful carrying out of this important and far-reaching section will not be easy, not only because of the inherent difficulties, but because it will lay a heavy hand on vested interests. To those doctors who think that the Act gives them adequate representation on the Insurance Committees and freedom from Friendly Society control, Mr. Loch's remark that "the Insurance Committee will thus be a committee consisting to a predominating extent of the insured persons themselves," must cause some misgivings. And when he adds, "It will be a matter of interest to follow the results of this measure and ascertain whether eventually, like the Workmen's Compensation Act, and possibly the Trades Board Act, it may have the effect of adding to the number of unemployed," we feel that Mr. Loch is in the ranks of the doubters.

ETIQUETTE AND LAW.

IT is important for the practitioner who is threatened with or who commences legal proceedings to draw a nice distinction between rules of etiquette and the rules of law. If he refuse to attend a patient "only that he may conform to tyrant custom" he may find that a court of justice takes a different view of the matter. In a case recently decided in the Clerkenwell County Court the limits of the professional rule that a practitioner shall not attend a patient who has already been prescribed for by another medical man were seriously considered. The facts were simple. The plaintiff, who was one of the medical officers of a friendly society, sought to recover 17s. 6d., his fee for attending a member of the society. He had attended on three days in succession, and then, declaring that the patient was better, had announced his intention of seeing him again a few days later. The patient was suffering from a sore throat. On the evening

[1] *The Annual Charities Register and Digest*. Twenty-first edition London: Longmans, Green, and Co. and Charity Organization Society. 1912. (Demy 8vo, pp. 1058. 5s. net.)

of the third day the patient's mother became anxious about him, and again saw the plaintiff, who reassured her. Subsequently, however, she called in the other medical officer of the society, who pronounced that the youth had quinsy, and told the mother to get the plaintiff to relieve it. He also told her to tell him that the case was urgent. The plaintiff refused to have anything further to do with the case, as another practitioner had been consulted. He did not agree that the matter was urgent. The second officer relieved the patient, and was paid 17s. 6d. by the friendly society acting for the defendant. In answer to the claim the defendant counter-claimed for 17s. 6d., apparently on the ground that he had been compelled to incur this expense owing to the refusal of the plaintiff to continue his treatment of the case. In giving judgement for the defendant on claim and counter-claim, the judge seems to have taken the view that the plaintiff should have accepted the view of the second doctor that the case was urgent, and should have continued his attendance. He was apparently of opinion that, unless there is urgency, one doctor is not entitled to treat another's patient without his consent; and that, if the patient chooses to consult a second doctor, the first is entitled to discontinue his attendance. He acted on the testimony of the second doctor, who, in the course of his evidence, said: "When there is a serious case, and no time to write letters to the patient's doctor, I think it is quite sufficient to tell him the case is urgent. Then the least he can do is to come and see the case. If it is not urgent, he can blow me up for it." Having regard to the fact that the judge stated that "urgency" was the point in dispute, the case may almost be cited as an instance of judicial recognition of the general custom; but the moral of the case is that the practitioner must not put too strict an interpretation upon the custom if he is the official attendant of the patient. It is interesting to notice that a rule to prevent "poaching" has long been recognized in the legal profession. Thus, with certain limitations, no counsel will appear for a client who has already consulted another member of the bar with reference to the same matter, unless the first counsel consents to waive his rights.

THE POPES AND MEDICAL SCIENCE.

THERE is a widespread belief, still generally accepted even by educated people, that the Church of Rome has always been opposed to the study of medicine, and that the supposed neglect of scientific research during the centuries which preceded the Reformation was entirely due to the ignorance and bigotry of mediaeval Churchmen. To refute this accusation that stout "defender of the faith," Dr. James J. Walsh, has published his striking series of essays on *The Popes and Science;*[1] and to those who still cling fondly to an exploded myth this most interesting and original book will probably be something in the nature of a bombshell. It was high time that some one took up the cudgels in defence of the principal, and in many cases the only, patrons of learning during the Middle Ages; and Dr. Walsh produces a vast and solid array of evidence to show the unhistorical character of the old legends of the suppression of science by the monks. The origin of these legends is easily explained by the hostile attitude adopted by many of the early Reformers, who lost no opportunity of blackening the fair fame of the Church they had left; their reckless accusations have been taken in all seriousness by those who came after them. Hence arose a mass of false tradition; and, as Dr. Walsh truly remarks, it is to "this unfortunate state of affairs, and not real opposition on the part of the Popes to science," that we owe the persistent belief in "the supposed opposition between the Church and science." How groundless this belief is may be seen from the most cursory survey of Papal history. Far from suppressing knowledge, the Popes fostered it by every means within their power; and the enlightened interest displayed by them in everything appertaining to the art of healing is proved by the fact that for centuries Italy offered unrivalled opportunities for the study of science in all its branches, and medical students from every part of Europe flocked across the Alps to enjoy the freedom and educational advantages they could not hope to obtain at home. Dissection, generally supposed to be under the ban of the Church, was more practised in Italy than in any other country in Europe, and, says Dr. Walsh, "the nearer to Rome the medical schools were, the more dissection was done in them." It was most common of all in Rome itself, and the famous Bull of Pope Boniface VIII, far from forbidding dissection, as has so often been maintained, is here shown to have no bearing whatever upon the practice of cutting up the human body for anatomical purposes. As a matter of fact, it was directed solely against the popular but eminently unhygienic custom of boiling the bodies of dead crusaders in order that the bones might be carried back from the Holy Land and buried at home. Pope Boniface surely merits praise rather than blame for his prompt recognition of what might have proved a menace to the public health. This care for the temporal as well as the spiritual welfare of those under their care was displayed by many other pontiffs besides Boniface VIII; and it is a significant fact that the first city hospital in the whole of Christendom was founded in Rome by one of the Popes, whilst the modern hospital system owes its being largely to the practical charity of the same man, the famous Innocent III. But this interest in medicine, manifested by so many of the Roman Pontiffs, ceases to be surprising when we remember what Dr. Walsh calls "the most striking feature of the relation of the Popes to medicine."

> If they really were the bitter opponents of things medical . . . then we should expect that either there were no such officials as Papal physicians, or else that the men who occupied these posts were the veriest charlatans, who knew very little of medicine, and certainly did nothing to develop it. As a matter of fact, there is no list of physicians connected by any common bond in history who are so gloriously representative of scientific progress in medicine as the Papal physicians. The faculty of no medical school presents such a list of great names as those of the men who were chosen to be the official medical attendants of the Popes, and who were thus given a position of prominence whilst their discoveries in medicine had a vogue they otherwise could not have attained.

With great justice he continues:

> Could the Popes possibly have done anything more than this for medicine, or shown their interest in its progress, or made people realize better, that while prayer might be of service, every possible human means must be taken to secure, maintain and recover health?

The close connexion which existed for centuries between science and the Papacy was no doubt partly due to the fact that, the universities being ecclesiastical institutions, most of the teachers and nearly all the students were clerics, whose scientific attainments, it is worth noting, did not prevent many of them from reaching high preferment in the Church and enjoying the friendship of Popes and cardinals. Two of the mediaeval Popes, Sylvester II and Victor III, had distinguished themselves, before their elevation to the Papal throne, by their knowledge of science, including medicine, whilst the author of a remarkable little book on diseases of the eye, Peter of Spain, one of the most distinguished physicians and scientists of the thirteenth century, ended his days as

[1] *The Popes and Science: The History of the Papal Relations to Science during the Middle Ages and Down to our Own Time.* By James J. Walsh, K.C.St.G., M.D., Ph.D., Litt.D., LL.D. Knights of Columbus edition (30,000). New York: Fordham University Press. 1911. (Pp. 431.)

Pope John XXI. These facts alone should be sufficient to prove the fallacy of the popular belief in the systematic persecution of science by the mediaeval church, and Dr. Walsh deserves the gratitude of all historians for his fearless exposition of an ancient and groundless calumny.

THE TREATMENT OF CANCER.

IN spite of material advances in the knowledge of the pathology of malignant diseases, it has to be confessed their etiology is still an unsolved problem. In view of the lack of exact knowledge as to how cancer and the allied diseases are produced, it is not surprising than an efficacious remedy, other than mechanical removal, is also still the object of search. By "mechanical removal" is meant the removal of the diseased parts together with the surrounding healthy tissues by operative measures, and also the selective removal of the diseased tissues alone by some physical agent, such as Roentgen rays. It is, therefore, interesting to learn that Dr. Adolf Zeller, formerly of Stuttgart, and now of Weilheim, in Würtemberg, has spent some years in attempting to reproduce in an improved form the treatment by means of silicates, which is said to have been instituted by Batty, and of the arsenio-mercurial pastes of Astley Cooper, Hutchinson, and Dupuytren.[1] At first he limited himself to giving the silicates internally to patients who were suffering from cancer. Realizing that a general practitioner has little opportunity of studying a disease and its treatment thoroughly, he, after a period of ten years, left Stuttgart and went to Heidelberg, where he worked under Czerny. His work was interrupted by a serious illness, and he was obliged to leave Heidelberg in August, 1908, to recruit his health. When he had recovered, he took up his abode in Weilheim, a village of some 3,000 inhabitants, where he devoted himself entirely to the investigation of cancer. Before discussing the cases treated in these three periods and the results obtained, it is necessary to mention that Dr. Vincent Czerny writes[2] from Heidelberg to the effect that he, together with some others, investigated the results obtained. In seventeen of the cases microscopical preparations of the tumours were available, and the diagnosis was confirmed by these. In the majority of the patients smooth, healthy scars were the only indications of the disease. Dr. Czerny states that in Heidelberg they had too little experience to speak of the effect of the silicic acid part of the treatment, but he is disposed to believe that the combined treatment is better than the paste treatment alone. He regards the treatment "as extraordinarily remarkable," and invites general practitioners to apply it. The patients in the first period were treated exclusively with silicic acid or one of the silicates. There were 9 cases, but in none was the diagnosis confirmed by microscopical examination. Four of these were superficial growths of the skin of the neck or face, while 5 were mammary cancers. One patient died, the diagnosis was doubtful in 2 others, while in the last 2 Dr. Zeller states that the clinical diagnosis left no room for doubt. Eight of this group of 9 were cured. In the second period 25 patients were treated. Among these no complete cures were effected, although it is stated that the disease was arrested and the growths became smaller. In the third period 57 patients were treated by the combined method. This consisted in giving sodium and potassium silicate in powder form internally for several months, and in applying a paste the composition of which is not set out, but which consists of arsenic and cinnabar, and which is similar to the old Cosme's paste. Of these 57 cases, 3 patients died, 10 are still under treatment, and the remaining 44 are said to be cured. In 20 cases the diagnosis was confirmed by histological examination. Ten of the tumours are said to have been deeply infiltrating, 22 of the 44 were skin growths of the head, face, or neck, 13 were growths of the nose and nostrils, 3 were breast cancers, 2 cancers of the cervix and three growths of the labia. A successful result by chemical removal applied externally is not altogether rare, and while the fact that temporary removal was effected in all the accessible cutaneous growths is highly satisfactory in itself, it must be pointed out that the time which has elapsed since the applications is too short to speak of cures, some of the periods being only one month. But the complete removal of the growths in the breast cases, and the production of smooth, healthy scars, even if this is only temporary, is certainly a noteworthy result. Professor Czerny will deliver an address on the effect of medicinal treatment of cancer at the meeting of German scientists and medical practitioners shortly to be held at Münster, and the description of Dr. Zeller's treatment will form a very interesting part of his speech. Until we learn more of this treatment, and especially until a very much larger number of cases have been treated in this manner, we must maintain the attitude of reserve which we have hitherto held in regard to the non-operative treatment of cancer and wait for fuller knowledge.

[1] Dr. Adolf Zeller, *Muench. med. Woch.*, August 20th and 27th, 1912.

[2] Introduction to Dr. Zeller's article by Dr. Vincent Czerny, *Muench. med. Woch.*, August 20th, 1912.

CAN THE UNDELIVERED FETUS CRY?

THERE has recently been some discussion in French and Belgian medical papers about the nature of the *vagitus uterinus*.[1] Is it the fetus or the uterus which causes the remarkable phenomenon occasionally heard before the fetus is delivered? Velpeau once said that even if he had heard them he would not believe in them, that is to say, he would not ascribe them to the fetus. Allard, on the other hand, has published an observation in the *Normandie Medicale* which leads him to believe that the *vagitus* is uttered by the fetus. Let it be remembered, however, that the forceps was used. It was a seventh labour in this instance, and on the three previous occasions the forceps had been applied, apparently on account of inertia. After a first attempt, which proved unsuccessful, he allowed the patient to rest before introducing the instrument once more. Suddenly he heard stifled cries, just like those of a new-born infant under the bedclothes, but the child was still undelivered. At the same time the nurse who was holding the patient's leg cried out, "*On voit bien qu'il s'ennuie, il pleure!*" and the mother drew herself up and exclaimed, "What's making that noise?" The husband, apparently a doctor himself, was present, and also heard the *vagitus*, which was repeated four or five times. The child's movements were vigorous. The forceps was again applied, but the child was born asphyxiated. It weighed over 11 lb. Unfortunately there does not appear to have been any *post-mortem* examination. Sippel, about eight years ago, reported a case of his own in which he undertook version. A pain occurred and he was obliged to stop his manœuvres without withdrawing his arm. Immediately two high-pitched notes were heard, clearly arising within the patient's abdomen. They precisely simulated the cries of an infant. But Sippel noted that at each cry air ran along his forearm engaged in turning, and he distinctly felt the vibrations of a fold of mucous membrane closely encircling his forearm. The current of air ran not from below, but from above downwards. The uterine contraction drove it out, and as it passed the slightly resistant fold the sound was produced. Possibly the *vagitus uterinus* may be explained in a similar way in most, if not all, other cases. There seems usually to be a flabby uterus on the one hand and an obstetrical operation admitting air into its cavity on the other.

[1] Editorial article in *Journ. d'accouchements et revue de méd. et de chir. prat.*, Liège, May 26th, 1912.

THE HOUR-GLASS STOMACH AND CORSETS.

IN an article on hour-glass contraction of the stomach S. and J. Nordentoft[1] discuss the view, which has lately been revived in Denmark, that the condition is oftener than not primarily due to pressure by corsets. This theory explains the greater frequency of the hour-glass stomach in women who, according to some observers, constitute 80 to 85 per cent. of the total number of such cases. Harslöff finds a still greater inequality in the distribution of this condition between the sexes, for out of 58 cases which were operated on he found only 1 male, and out of 61 cases in which an hour-glass stomach was found at necropsies he found only 3 males. Gastric ulceration in its severer forms is, on the other hand, commoner in men than in women, and therefore the view that the hour-glass stomach is usually caused by gastric ulceration appears to be at fault. The process by which corsets may lead to the formation of an hour-glass stomach probably consists of a narrowing of the lower aperture of the chest, which favours gastroptosis or, if a strong abdominal wall prevents the downward displacement of the stomach, the kinking of the stomach backwards on its long axis. Such a process occurs either at the junction of the fundus with the body of the stomach, or in the region of the pylorus; and it leads to the adhesion of the two peritoneal surfaces, a crest being formed which projects into the stomach at its lesser curvature. Ulcers may, it is true, form on this crest, when their cicatrization will favour the development of the hour glass, but in such cases ulceration is clearly a secondary factor. The writers recognize two types of hour-glass stomach according as they are produced by corsets or primary ulceration of the stomach. In the first class, the distance between the pyloric and cardiac ends of the stomach is diminished, an acute angle being formed by the kink which involves the lesser as well as the greater curvature of the stomach, or even the lesser curvature alone. In the second class, the distance between the two ends of the stomach is unaltered, and the constriction is almost confined to the greater curvature. Rovsing, of Denmark, goes so far as to state that while the hour-glass stomach is occasionally congenital or primarily due to gastric ulcer, it is, as a rule, caused by corsets or belts, which contract the lower aperture of the chest. In most of the 10 cases which he has reported, the constricted portion of the stomach was lined by a normal mucous membrane, ulcers being seldom found covering this area. Rovsing's interpretation may be true enough for his own cases, but in the majority of cases reported by others the constricted area was seldom covered by normal tissues. Thus, out of 133 cases collected by the writers, there were only 14 in which no gastric ulcers were found. Whatever the cause may be, the hour-glass stomach which has given rise to symptoms can be effectively treated only by an operation. Numerous operations have been devised, and, owing to the various complications, such as pyloric stenosis and adhesions between the stomach and neighbouring viscera, which may be found at the operation, no one operation is suitable for every case. In the case of a woman, aged 53, whose stomach was shown by the *x* rays to be constricted in the middle by an extremely narrow isthmus, the writers performed gastro-enterostomia antecolica anterior, combined with an entero-anastomosis. This operation effected not only a communication between the two sections of the hour glass, but it also provided a new passage from the stomach into the intestine, a large part of the stomach in its most dependent area being supplemented by a portion of small intestine. The patient, whose condition had incapacitated her from all work, made an uneventful recovery, her weight rising from 38.5 to 46.5 kilos, and symptoms, such as indigestion, disappearing completely.

[1] *Hospitalstidende*, 1 and 2, 1912.

THE FIELDS OF VISION IN TABES.

ON July 11th, 1911, Professor Fuchs delivered a lecture to the American Ophthalmological Society at New London, which is published in the *Archives of Ophthalmology* for September, 1911, upon "The Field of Vision in Tabetic Atrophy of the Optic Disc." The first class of cases noted were those, estimated by Uhthoff as about 2 per cent. of all cases of tabetic atrophy, in which there is a central scotoma. Fuchs believes that they are more common than Uhthoff's figure. He thinks that this central scotoma is not due to a simultaneous syphilitic retrobulbar neuritis, but is an essential part of the tabetic process. It is nearly always bilateral and is at first accurately central. It develops by joining the already enlarged blind spot of Mariotte. Eventually it assumes the shape of a horizontal ellipse. At first only the perception of red and green are abolished, but ultimately the scotoma becomes absolute. Even when it is relative we can, by Bjerrum's method, detect islets of absolute blindness. The scotoma not only becomes absolute, but increases in size. At the same time peripheral contraction of the field, especially for colour, sets in, and eventually colour perception may completely fail. The peripheral contraction becomes irregular, an indentation appears, and a large area becomes totally blind. These peripheral changes serve to distinguish a tabetic case from one of tobacco amblyopia in which peripheral vision for form and colour is unaffected. It is more difficult to separate these tabetic cases from one of syphilitic retrobulbar neuritis, especially as this condition may be associated with loss of reflexes and an Argyll-Robertson pupil. Fuchs knows of only two distinctive features which favour the diagnosis of syphilitic neuritis. First of all the influence of appropriate treatment which succeeds in arresting the loss of sight, and, secondly, the fact that it retrobulbar neuritis the pallor of the disc is likely to develop at a later stage. Another exceptional type of alteration of the field of vision in tabes is bitemporal hemianopsia. These cases are rare. The phenomenon indicates a lesion of the decussating fibres situated at the inferior side of the chiasma. It occurs in the case of a tumour pressing upon the inferior surface of the chiasma, as in tumours developing from the pituitary gland. It may also result from a local inflammation, mostly syphilitic, of the developing membranes of the brain at the same place. Such an inflammation has also been supposed to cause the bitemporal hemiopia in tabes. Fuchs admits that this may here and there be the case, but in general he believes that we have to do with the same kind of grey degeneration which is characteristic of tabetic atrophy in general.

MEDICAL WOMEN IN GERMANY.

IT is stated that there are now 175 qualified medical women in Germany. Particulars as to 125 of them have been collected by Frau Dr. Stelzner. The results of her inquiry show that of that number 78 are unmarried, and 47 married (including five widows). Of the married women 34 have given up practice; two of them, however, who are wives of doctors, assist and act for their husbands. Considerably more than half the number are married to doctors. Two began their studies after marriage. The women who practise are distributed in the cities of Germany as follows: Berlin, 30; Munich, 7; Frankfort-on-the-Main, 5; Hamburg, 3; Düsseldorf, Breslau, Karlsruhe, Heidelberg, and Bad Elster, each 2; while a number of other places have one each. Most of the women doctors in Germany practise as specialists, especially in diseases of women and children. During the last few years the chances of medical women have steadily become more favourable in Germany. They often secure posts in hospitals, institutes, and asylums. Those entering the profession may, it is said, count with fair certainty that they will be able to earn a sufficient income within a year or two of qualification.

TWINSHIP AND FAME.

IN a paragraph headed "From Triplets to Sextuplets," which appeared in the JOURNAL of April 6th, 1912, p. 802, reference was made to Dr. Kaiser, of Dresden, who stated that he knew of no famous man who had a twin brother. The question had been referred to our contemporary, *Notes and Queries*. It appears that the same query had already been raised in that useful serial in 1862, owing, as now, to an assertion, identical with Dr. Kaiser's, made by Dr. Simson in the *Edinburgh Medical Journal*. Simson was not aware of a single instance in which a twin had distinguished himself intellectually. The Rev. A. B. Grosart demurred to this assertion, pointing out that Henry and Thomas Vaughan, twins born on April 17th, 1622, both distinguished themselves. Since Grosart wrote his observations both have been immortalized by Sir Sidney Lee in his *Dictionary of National Biography*. Henry was the "Silurist," so called because a native of the district anciently inhabited by the Silures, which included Brecknockshire, where H. Vaughan was born. In 1645 he was practising as a physician in Brecknock, but his fame was won in the realm of poetry. Thomas, his twin brother, became an alchemist, but wrote some poetry of acknowledged merit. We doubt, however, if the brothers Vaughan reflect great glory on twinship. With the Scotts it is otherwise. Lord Stowell and Lord Eldon were "pigeon-twins," each entering the world with a sister. William Scott, "hostman" or coal fitter of Newcastle, father of the famous pair, had three children by his first wife and thirteen by his second. The latter bore him twins three times: William (afterwards Lord Stowell) and Barbara (1745), John (afterwards Lord Eldon) and Elizabeth (1751), and, about 1754, a son and daughter stillborn. Barbara lived to the age of 78, but Elizabeth died a fortnight after her birth. The most distinct feature about the Scotts is that they were both "pigeon-twins" and therefore not uni-oval. We know that in the case of the one-yelk twins they are always of the same sex. We also know that one at least is necessarily exposed to conditions which may more or less retard its development, and may greatly prejudice the other twin. Acardiac and all "Siamese" twins, two-headed fetuses and the like, are of the uni-oval and uni-sexual class. It is not certain, however, that the cerebro-spinal system is necessarily prejudiced when one uni-oval twin attains maturity, nor, indeed, is it certain when both do so. There is therefore no embryological nor psychological reason why even a "true twin," as an embryologist would call it, should not live to become famous. As for pigeon-twins, they are independent beings, and it is only in parturition that they are in jeopardy. When, however, we turn back to the original question, the reply is that, as far as records can teach us, twins of both kinds very rarely attain fame. The Vaughans, being both males, were possibly true one-yelk twins, but twins of the same sex may be developed from separate ova, and consequently are not troubled and endangered by a common placenta, hence the Vaughans more probably came under this class. The Scotts were clearly of the two-yelk type. Still, again, we know of no other famous "pigeon-twins." Out of the only two pairs clearly authentic, one doctor is to be found. Has any other celebrated member of the medical profession's twinship been overlooked? In conclusion, registration of births was faulty down to recent times, so a case or two of a twin who made his mark in the world may have escaped the vigilance of biographers.

IT is stated in the American journals that President Taft has signed the Shirley Bill, which makes more drastic the pure food and drugs law as applied to patent and proprietary medicines. "The bill prohibits, under a heavy penalty, the publishing of claims for 'cure alls' that cannot be substantiated, or which have no foundation in fact. The legislation is of importance to medicine manufacturers, and gives the Government a much greater authority over the trade in nostrums."

THE INSURANCE SCHEME.

STATE SICKNESS INSURANCE COMMITTEE.

THE third meeting of the State Sickness Insurance Committee appointed by the Annual Representative Meeting, 1912, was held on September 5th, and sat, with an interval of half an hour for lunch, from 10.45 a.m. to 9.20 p.m.

Mr. VERRALL, the Acting Chairman of the Committee, not being present, the PRESIDENT OF THE ASSOCIATION (Sir James Barr) was elected to the chair for the meeting. The members present were: *England and Wales:* Dr. R. M. Beaton (London), Dr. T. M. Carter (Westbury-on-Trym), Dr. Major Greenwood (London), Dr. S. Hodgson (Salford), Miss Frances Ivens (Liverpool), Dr. Constance Long (London), Dr. E. J. Maclean (Cardiff), Dr. James Pearse (Trowbridge), Dr. E. O. Price (Bangor), Mr. D. F. Todd (Sunderland), Mr. E. H. Willock (Croydon). *Scotland:* Dr. J. Adams (Glasgow). *Ireland:* Dr. J. S. Darling (Lurgan).

APPOINTMENT OF CHAIRMAN OF COMMITTEE.

Dr. J. A. MACDONALD, Chairman of Council, was elected Chairman of the Committee.

CO-OPTION TO THE COMMITTEE.

Dr. Lauriston E. Shaw (London), and Mr. Herbert Jones, Honorary Secretary to the Society of Medical Officers of Health (Hereford), were co-opted as members of the Committee.

QUESTIONS ARISING IN CONNEXION WITH SANATORIUM BENEFIT.

The greater part of the day was taken up with the consideration of correspondence in connexion with various questions concerning the administration of sanatorium benefit.

Relation of Medical Officer of Health to Post of Chief Tuberculosis Officer.

It was reported to the Committee that, acting on instructions of the Acting Chairman of the Committee, an advertisement had been inserted in the BRITISH MEDICAL JOURNAL for a tuberculosis medical officer for Deptford at a salary of £300 per annum rising to £350, the successful candidate being attached to the Public Health Department and being required to work under the administrative control of the M.O.H. In connexion with this advertisement and with the question of principle involved in it, the Committee considered several letters which had been received and which had influenced Mr. Verrall in sanctioning the insertion of the advertisement. These letters pointed out that there were some districts in which the amount of work in connexion with tuberculosis would not justify the payment of a salary of £500 per annum, and advised the Committee that an attempt to insist on such a salary would, if successful, be a great temptation to local authorities to insist on the tuberculosis officer undertaking treatment, so lessening the amount of work open to the local practitioners. After careful consideration of the whole question in the light of the following minutes of the Representative Meeting which the Committee had previously decided should be regarded as cardinal points—

> That the chief tuberculosis officer should be a whole-time officer and confine himself to diagnosis and consultative work. The rest of the staff of the dispensary should, where possible, be formed of local medical practitioners serving on a rota or otherwise;
>
> That the commencing salaries for whole-time medical officers engaged in the Tuberculosis Service should be for junior or assistant whole-time medical officers not less than £300 per annum, and for senior whole-time officers not less than £500 per annum. These salaries in all cases must be exclusive of travelling and other official expenses—

the Committee, with two dissentients, passed the following resolution:

> That the Chief Tuberculosis Officer should be a whole-time officer, and should confine himself to diagnosis and consultative work, but, nevertheless, the State Sickness Insurance Committee recognizes that in certain cases it may be expedient for the medical officer of health to perform the *administrative* duties of such office until further and better arrangements can be made; and, therefore, it is

inopportune for the Association to insist at the present time on advertisements accepted for insertion in the JOURNAL in all cases implying the appointment of a chief tuberculosis officer at a salary of £500.

The action of the Acting Chairman in sanctioning the insertion of the Deptford advertisement was approved.

Mileage Fees in Connexion with Domiciliary Treatment.

Various correspondents having drawn attention to the fact that the scale of fees issued by the Committee in connexion with the domiciliary treatment of tuberculosis contained no reference to mileage, the Committee resolved that the fee should be 1s. per mile beyond the first two miles.

Capitation Fee for Domiciliary Attendance.

It was reported that suggestions had been made by various Provisional Insurance Committees that a capitation equivalent might be accepted for the fees authorized in connexion with domiciliary treatment. In view of the following Minute 205 of the Representative Meeting, the Committee resolved by 8 to 2 that it could not advise the acceptance of a capitation fee.

> That the payment to be made to medical practitioners for domiciliary attendance on patients certified to be suffering from tuberculosis shall be on a scale of fees and not by capitation.

Provision of Medicines in Connexion with Domiciliary Treatment.

Inquiries having been made as to whether the scale of fees laid down for domiciliary treatment included the provision of medicines, the Committee passed the following resolution:

> That the Committee is of opinion that the question of whether medicines should or should not be provided for the fees adopted in respect of domiciliary attendance is one for local decision, as the Representative Meeting arrived at no decision on the subject.

Practitioners and Tuberculin Treatment.

Strong remonstrances having been received as regards the assumption which it was alleged that the Association had made—namely, that tuberculin treatment was a matter which any practitioner could carry out without any special experience—the Committee directed that correspondents be informed that the Committee had always assumed that tuberculin and other forms of treatment would be carried out by general practitioners in association with the Chief Tuberculosis Officer.

Salaries of Chief Tuberculosis Officers in Ireland.

Correspondence was received as to the advertisement in certain Irish counties of appointments as Chief Tuberculosis Officers at salaries considerably below the minimum established by the Representative Meeting.

It was also reported that the Joint Committee of the British Medical Association and Irish Medical Association had agreed that salaries not below £400 per annum might be accepted in Ireland for the post of Chief Tuberculosis Officer. The Committee expressed its disapproval of this action of the Joint Committee, and its regret that the salaries reported as having been offered for these important posts had secured any applicants.

Schemes for Administration of Sanatorium Benefit.

A large number of schemes for administration of sanatorium benefit had been forwarded for the consideration of the Committee, but it was found to be impossible to deal with more than a few of them. The remainder will receive consideration on September 12th.

RESIGNATION OF CONTRIBUTORY CONTRACT PRACTICE APPOINTMENTS.

The Committee had before it a report from a considerable number of those Provisional Medical Committees whose areas had been considered either "weak" or "dangerous" from the point of view of an early resignation of all contributory contract appointments concerning insured persons. The report showed that considerable progress had been made in the organization of these areas, though a few had not replied to the inquiries of the Committee. Various questions of detail were considered and a draft letter to the local committees dealing with the whole question was ordered to be prepared for the next meeting of the Committee on September 12th.

SUGGESTED RESIGNATION OF MEDICAL APPOINTMENTS IN THE TERRITORIAL FORCE.

The Committee had brought to its notice a report which had obtained considerable publicity, that a Provisional Medical Committee had recommended that medical officers in the Territorial Force should resign their commissions as a protest against the medical provisions of the Insurance Act. The Committee expressed its strong disapproval of such a suggestion as being calculated to alienate public sympathy from the profession.

PUBLIC MEDICAL SERVICE SCHEMES.

The Committee considered the minutes of four meetings of the Public Medical Service Subcommittee, which recommended three schemes for adoption by the Committee and transmission to the Divisions. The Committee spent a good deal of time in considering these schemes in detail, and approved two of them after making certain amendments. It was found impossible to deal with the third, but it was arranged that this should be done at the commencement of the meeting on September 12th, in order that those schemes which were approved might appear in the SUPPLEMENT of September 14th (see page 290).

RESIGNATIONS FROM ADVISORY COMMITTEES.

It was reported that since the last meeting of the Committee information had been received that the following had resigned their positions on the Advisory Committees, in accordance with the decision of the Representative Body: Dr. R. C. Buist (Dundee), Dr. T. B. Costello (Tuam), Mr. Herbert Jones (Hereford), Dr. J. E. Moorhouse (Stirling), Professor R. Stockman (Glasgow), Dr. Lauriston E. Shaw (London), Miss A. H. Boyle (Brighton), Dr. Constance Long (London), Dr. A. Mercer Watson (Aberdeen).

The following have intimated their intention not to resign at present: Dr. C. Addison, M.P., Dr. J. Robertson (Birmingham), Professor Sims Woodhead (Cambridge), Sir John Collie (London), Sir T. Clifford Allbutt (Cambridge), Dr. G. Reid (Stafford), Dr. A. Hamilton (Luton), Dr, G. Belcher (Birmingham). Dr. A. Latham (London)

No reply has been received from the following: Dr. Adam Fulton (Nottingham), Dr. W. E. Thomas (Ystrad Rhondda), Mr. A. Crook (Norwich), Dr. R. A. Lyster (Winchester), Miss M. C. Murdoch (Hull), Right Hon. M. F. Cox (Dublin), Dr. Hugh Jones (Dolgelly), Mr. D. N. Morgan (Bridgend), Dr. J. D. Jenkins (Rhondda).

CENTRAL INSURANCE DEFENCE FUND.

The Committee received a report on the Fund showing *inter alia* that since the Annual Representative Meeting the amount guaranteed had increased by £12,788. The Committee expressed the hope that a more rapid increase would occur after the termination of the holiday season.

AN epidemic of poliomyelitis is reported in Southern Sweden. The number of deaths from the disease since January 1st is 1,459.

IT is some testimony of the progress of the garden city movement that the publication of a periodical named *Garden Suburbs, Villages, and Homes*, which first put in a fleeting appearance some six years ago, has now been recommenced. The current issue contains some forty or fifty reproductions of pictures and photographs, which between them give a good idea of the garden suburbs and villages in being, while the letterpress is by various well-known writers. From one paper it appears that in 1906 there were only four societies engaged in promoting the movement, and only three of them possessed any property; whereas there are now fourteen, who between them possess 647 acres, providing room, it is estimated, for 6,595 habitations. It is published by the Co-Partnership Publishers, Ltd., 3 Southampton Row, London, price 6d. net.

Ireland.

[FROM OUR SPECIAL CORRESPONDENTS.]

SANATORIUM BENEFITS.

THE following statement from the Conjoint Committee of the British Medical and Irish Medical Associations has been sent to the daily press, explaining the attitude of the medical profession in Ireland towards the sanatorium benefits of the National Insurance Act.

The Sanatorium Subcommittee of the Conjoint Committee desire to assure the public that the medical profession in Ireland are anxious to give their most hearty co-operation in the administration of the sanatorium benefits, and to take their full share in the prevention of tuberculosis. The chief reason for the resignation of the medical members of the Irish Advisory Committee was due to the fact that the regulations for the conduct of the sanatorium benefit were far advanced before the medical members of the Advisory Committee were consulted. The Irish Insurance Commissioners gave the Conjoint Committee a promise that if each Local Medical Committee send forward four names the two medical members of the Insurance Committee would be selected in each case from these four names. In some instances this pledge was not fulfilled, and in these cases only were the medical members asked to resign. The subcommittee desire to point out that the regulations for the administration of sanatorium benefit have not yet been issued by the Irish Insurance Commissioners, and that pending the issue of these regulations and definite arrangements under them there is no barrier to the attendance on tuberculous patients or to the signing of certificates recommending patients for sanatorium benefits. The committee trust all necessary treatment and certificates will, until then, be readily granted.

DOMICILIARY TREATMENT OF CONSUMPTION IN CO. CORK.

The Secretary of the Cork County Insurance Committee was deputed to wait on the Secretary of the County Medical Association with a view to arranging the fees to be charged by the profession for attendance on consumptive patients in their homes. It was decided that the terms of the profession should be 10s. for an inspection and report, and 5s. per visit for persons recommended for domiciliary treatment. It appears this raises the question of the available finances of the Insurance Committee, as their funds would be about £4,200, and there are about 80,000 insured persons in the county. The Commissioners are to be communicated with on the subject.

PEAMOUNT SANATORIUM.

A visit of inspection was paid recently to the Peamount Sanatorium by the Countess of Aberdeen, the members of the Provisional Committee of Superintendence, and others. The sanatorium is nearly completed, and it is expected that it will be ready for the reception of patients very shortly. One of the chalets was almost ready, a second practically completed, and the third is in an advanced stage. There are twelve open-air chalets for those patients undergoing treatment which necessitates sleeping in the open-air. The members of the Provisional Committee expressed their great satisfaction with the location of the sanatorium and with the arrangements for the reception of tuberculous patients.

MEDICAL CONTRACT PRACTICE IN LIMERICK.

The Honorary Secretary, Limerick Federation of Friendly Societies, recently published a long letter in the daily papers with the object of laying before the Irish public a statement of the attitude of the societies in that city towards the new medical regulations which were issued by the Limerick doctors last May. After recapitulating the six rules drawn up by the medical profession in Limerick, the letter goes on:

As the press recently has shown the views of the Medical Association and its critics, a sober *résumé* of the facts from the societies' standpoint may have the dual advantage of impressing the profession in Ireland with a sense of the serious responsibility incurred should a fight be forced on the societies, and the certain impossibility of such a fight ending in any other way than absolute defeat and confusion of the members of the profession; secondly, it may serve to encourage societies placed in remote parts of Ireland to resist, although no course may now seem possible except submission.

As the result of a round-table conference between the Federation and the Medical Committee, the date when the new regulations would come into force was postponed from July 15th to January 1st, 1913. It is stated in the letter that there is no instance in which any medical officer of a society in Limerick has ever complained of his remuneration or of the conditions under which he not alone accepted, but fought to secure the appointment. A member of a friendly society, before the Insurance Act date, paid in Limerick on an average 6s. per annum for medical attendance only, which included his dependents under 16 or parents, and about 2s. 6d. extra for medicine. The letter also states that the Federation has been informed that medical benefits were withheld from Ireland at the request of the medical profession, and that this has not been contradicted. The simple explanation is that such a monstrous falsehood has never been suggested before, and the Honorary Secretary of the Conjoint Committee of the British Medical Association and Irish Medical Association has immediately denied it in a letter to the press. The letter concludes by saying that "the societies are in no case anxious to provoke a quarrel and to make difficulties for an amicable understanding to be arrived at, but the societies have adopted that attitude not from any sense of weakness or lack of organization, but knowing the strength and justice of their case and the ease with which the Medical Association and its threats of boycott and intimidation can and will be set at defiance." Federations of friendly societies are in course of formation in Belfast, Dublin, Cork, and other places, and at an early date a convention of these federations is to be held in Dublin.

INSANITY IN IRELAND.

The annual report of the Inspector of Lunatics on the District, Criminal, and Private Lunatic Asylums in Ireland for 1911 shows that the insane in establishments on January 1st, 1912, numbered 12,868 males and 11,787 females, an increase of 261 in the twelve months. These numbers do not include the insane wandering at large or those residing in private dwellings, with the exception of Chancery patients and a few others who are specially returned. This increase was 61 less than the average increase for the preceding ten years. Since 1880 there has been a decrease of 956 in the pauper lunatics in the workhouses. The number of the insane under care has increased from 250 per 100,000 of the population in 1880 to 563 per 100,000 in 1911. The average rate of increase for the past five years has been over 5 per annum, whereas the average rate during the entire period was over 10 per annum. The distribution in the various counties is very unequal, varying from 2.6 per 1,000 in County Down to 9.2 in Waterford. Upon the whole it appears that insanity tends to prevail in the agricultural and rural counties. The order of the four provinces is: Munster 6.1 per 1.000, Leinster 6.0, Connaught 5.2, and Ulster 4.2. There is no appreciable connexion to be detected between the density of the population and insanity, nor can any distinct relationship be traced between the latter and the rateable valuation of the land. As regards pauperism, however, the case is different, as there would appear to be a close relation between the amount of pauperism and insanity in any district; thus Waterford, which gives the highest rate of insane in institutions, gives also the highest but one of pauperism, and all but two of the counties which stand over the average in pauperism were also over the average in insanity. It would appear that the influence of emigration in increasing insanity has been exaggerated, as when the emigration rate and the insanity rate are compared, county by county, it is found that there is no marked degree of relationship between them. Alcoholic addiction occurs in two forms, not necessarily distributed alike, namely, drunkenness and chronic alcoholism, of which the former, unfortunately very prevalent in Ireland, is undoubtedly much the less serious as a cause of insanity. From the statistics available regarding drunkenness, it would appear that there is practically no relationship between the distribution of insanity and drunkenness in this country; and the total amount of chronic alcoholism is so small that it can have no great influence on the insanity rate. There is some slight relation between the general distribution of insanity and the percentage of admissions ascribed to alcohol, so far as they can be compared, but the general conclusion which may be safely drawn from the facts is that alcohol possesses comparatively small importance as a cause of insanity in Ireland.

Dublin Hospitals Report.

The annual report for the year 1911–12 of the Board of Superintendence of the Dublin Hospitals has been issued recently. This report only refers to those hospitals—nine in number—which receive grants from Parliament. The report states that all these hospitals were visited and fully inspected, and it was found that the wards were clean and well ventilated; every provision was made for the treatment, comfort, and welfare of the patients, and the several buildings were kept in good repair and in a sanitary condition. Inquiries were made respecting the current expenses of each hospital and the system of purchasing, storing, cooking, and issuing supplies. The nursing arrangements were also examined, as also the means available in each hospital for the extinction of an outbreak of fire and for the escape of the inmates in the event of such an occurrence. In an appendix are tables showing the work done and dealing with the cost of working each hospital. A study of these tables, taken in conjunction with the evidence obtained on their visits to the several hospitals, enables the members of the board to report that the hospitals fulfil in an economical and thorough way the objects for which they were instituted. They conclude by saying, in regard to the Insurance Act, that its probable effect on charitable institutions is still speculative, but that it is plain that many questions of great importance will arise on which they will be in a better position to report next year.

Census Returns.

According to the census returns recently issued for Belfast the population has increased during the last ten years by 10.82 per cent. The one-roomed tenements number 448. The number relieved by the Poor Law has risen since 1901 from 1 in every 96 of the population to 1 in every 94. The number of emigrants during the last ten years was 32,804. In County Antrim there has been a decrease in the population of 2,226. The number of persons relieved by the Poor Law has fallen from 1 in every 63 of the population in 1901 to 1 in every 79 last year. In County Down the population has decreased by 1,586. Though there was an increase in eleven county districts and a decrease in only six county districts, the number relieved by the Poor Law has fallen in the last ten years from 1 in every 75 of the population to 1 in every 116. In the past ten years there have been 15,709 emigrants from the county.

The population of Connaught has decreased by 5.56 per cent. The number in receipt of Poor Law relief has fallen from 1 in every 55 to 1 in every 77 during the last ten years. The number of persons returned in 1911 as being able to speak the Irish language was 217,087, or 35.5 of the total population; of these, 9,367 could speak "Irish only." In Munster the population has decreased by 3.78 per cent. Those relieved by the Poor Law have decreased from 1 in every 29 to 1 in every 41 of the population. The number able to speak Irish is 228,694, or 22.1 per cent. of the total population; of these 2,766 could speak "Irish only."

Scotland.

[FROM OUR SPECIAL CORRESPONDENTS.]

Early Travellers in Scotland.

A paper on this subject was read at a recent meeting of the vacation course series of lectures at the University of Edinburgh by Mr. R. Scott-Moncreiff, W.S. He began with Froissart's references to the complaints of the French knights on reaching that country, and to the primitive fare and general want of comfort. Aneas Sylvius Piccolomini, afterwards Pope Pius II, who visited Scotland in the reign of James I, was wrecked near Dunbar. He was washed ashore, and made a vow to walk to the nearest sanctuary barefoot. This turned out to be the sanctuary of White Kirk, and Piccolomini had to walk ten miles, with frost on the ground. As a result he contracted rheumatism, which never left him, and he had to be carried on the return journey in a litter. Mr. Scott-Moncreiff quoted Piccolomini's famous account of geese that grew on trees, of which three that fell into the water swam away, while those that fell on the ground never came to life. He also told how the poor received doles of stones, which contained some fatty matter that burned; the "stones" referred to were evidently coal. He misconstrued the law relating to crows. He stated that the crow was so rare, that any tree on which it built belonged to the king. The opposite was the fact, the crows being so numerous that the law confiscating any tree in which they built was devised with a view to the restriction of their number. Ayala, another early visitor, gave a characteristic sketch of James IV, who, he stated, was courageous even more than a king should be. "He is not a good captain, because he begins to fight before he has given orders." Peder Swave, a Dane, described a great many wonders he had seen in Scotland. Not far from Edinburgh, he wrote, "there is a mountain which constantly smokes like Etna. I saw it myself." He also described how, at a monastery, oil flowed out of a well, probably the Balm Well of St. Catherine, near Burdie House. Fynes Moryson, an Englishman, in his account of Edinburgh, commented on the poor fare even of the better classes, and their want of what, from the English point of view, were considered the necessaries of life. Sir Antony Weldon (1617) expressed wonder that "so brave a prince as King James should be born in so stinking a town as Edinburgh in lousy Scotland." Sir William Brereton complained of the noisome condition of the houses. He also states that he saw the floating islands of Loch Lomond, where the fish were without fins. Richard Francis, who came first with Cromwell and afterwards returned on an angling tour, was, perhaps, the first to eulogize the scenery of Scotland; his book therefore deserved the honour it received in 1821, when it was edited by Sir Walter Scott.

Medical Conditions in the Highlands.

After hearing evidence in Kirkwall on August 26th (see British Medical Journal, August 31st, page 518) the Medical Service Committee crossed over to Lerwick on August 27th. On the way they were able to effect a landing on Fair Isle to verify on the spot information which had previously been supplied as to the arrangements for medical attendance and nursing for this isolated community. Most interesting evidence was given by the Queen Victoria Jubilee Institute's nurse resident on the island, and by the teacher and missionary, as to the means adopted for the treatment of cases of severe illness or accident in the absence of the medical man. On August 28th a meeting of the committee was held at Lerwick, at which representative witnesses, including medical practitioners, from all parts of the extensive Shetland group of islands were present. Here, as in Orkney, the outstanding difficulties were the poverty of the bulk of the people and the inaccessibility of many of the islands, and more particularly of the distant island of Foula, except in fair weather, a condition comparatively rare, mainly on account of the abnormal strength of the tides. Several witnesses gave vivid accounts of the dangers encountered by the doctor in endeavouring to bring medical aid, and of the suffering frequently entailed by his being storm-stayed. The majority were of opinion that the betterment of the existing conditions must be effected, not so much by increasing the number of medical practitioners as by providing a seaworthy motor-boat for the doctor, small hospitals, better and more ready means of communication by telephone, and by increasing the number of fully qualified nurses, and also the doctor's income. Attention was also drawn to the strong contrast between the medical and the ecclesiastical provision in the islands, and especially in the island of Yell.

Under the will of the late Mr. Graham Vivian, of Clyne Castle, Swansea, a legacy of £10,000 is to be paid to the funds of Swansea Hospital.

The King has granted Mr. Robert John Gordon, M.A., M.B., B.Ch., of the Irish Presbyterian Mission, licence and authority to accept and wear the insignia of the Third Class of the Order of the Double Dragon, conferred upon him by the Emperor of China in recognition of valuable services rendered as Plague Prevention Medical Officer at Hulan, Heilungchiang.

Correspondence.

THE HISTORY OF MEDICINE.

SIR,—In the second notice of Dr. Neuburger's *History of Medicine* the author is said to assert that: "In the Hippocratic Oath, the highest ethical standard of antiquity, the physician is enjoined to refuse to give his aid to the incurable. On the other hand, Christianity made it a moral duty for a doctor to give the benefit of his skill even in such cases."

There is no such injunction in the oath, and the statement seems rather a libel on the ancient physicians. For, though the rhetorical author of the *De Arte*, who was probably not a physician, repeatedly states that medicine does not take in hand incurable diseases, this seems merely his way of saying that a scientific physician recognizes the supremacy of natural causation and claims no supernatural power.

It may be a hit at the temple miracles, so strangely ignored by all the Hippocratic writers, and it is certainly parallel with the famous saying of Herophilus: "The most perfect physician is he who distinguishes between the possible and the impossible," and with Plato's remark that the wise physcian, like the wise pilot, "knows intuitively his own powers, and attempts only what is within their limits" (*Republic*, p. 361). That the ancient physician was enjoined to do his best, even for incurable patients, is evident enough from the *De Articulis*, § 58, though he is warned of the loss of reputation he may incur among the vulgar; and that he did not avoid patients unlikely to recover, or even the publication of his failures, is clear from the cases recorded in the Hippocratic collection, the great majority of which end fatally. On the other hand, the danger and disgrace which may be incurred by undertaking desperate cases and the advisability of refusing to have anything to do with them are commonplaces of mediaeval medical writings, though, doubtless, doctors of all ages and religions have considered it a moral duty to relieve suffering so far as they were able.—I am, etc.,

Torquay, Aug. 31st. E. T. WITHINGTON.

*** Dr. Burgess of Hoylake, and Dr. R. J. Ryle have also written to us on this subject. We wrote in our haste, and the error is the less excusable since we published an article on the Hippocratic Oath, in which the full text was given, in the JOURNAL of May 4th, 1912 (p. 1032). The passage referred to is in *De Arte*, which is included among the Hippocratic writings. We quote what was said in the article: "Apparently the Hippocratic physician deduces from this realization of the limits of art—a conclusion foreign to our ideas—that help should be withheld from the incurable. Medicine is to him the art 'whereby sufferers may be entirely freed from their ailments and severe attacks of disease mitigated, but which should be refused to those persons who are already overwhelmed by illness, since it is clear that in such cases art can effect nothing.' [The quotation is from Neuburger.] This teaching held sway to the Middle Ages, and probably much later. Mondeville expressly enjoins his pupils not to undertake difficult or hopeless cases, on the ground that this would damage their reputation."

THE MENTAL DEFICIENCY BILL.

SIR,—Your temperate leading article of August 24th emboldens me to offer a few explanations.

1. Only an inadequate and therefore misleading report of my paper has appeared in the press reports of the Catholic Congress. If you were willing to print the paper it might provide a platform of discussion for those who wish to see all the data of a difficult subject. If this is impossible your readers might be glad to have a sober scientific statement of our position.

2. We admit a "problem" of feeble-mindedness, inasmuch as feeble-mindedness is often coupled with neglect, cruelty, or destitution. But we do not agree with those who propose that certain difficulties, which are largely economic or ethical, should receive a drastically eugenic solution.

3. The vexed question of "natural rights," to which so much of your leading article was devoted, is not essentially medical, but ethical. It requires a fuller and more expert treatment than I can command in this letter.

4. The leading article admitted that the definitions of the bill are defective, and that eugenists (whose theories are the theories of the bill) desire "fuller knowledge."

We merely add to this frank admission the wish that until such fuller knowledge is reached punitive legislation should be stayed.

The bill has already passed its second reading. No authoritative body has been so alert to the medical, economic, social, legal, and ethical principles at stake as to question it, until the National Catholic Congress at Norwich, admittedly a body with much expert knowledge, condemned the bill. And as the bill was like to be hurried through Parliament, the condemnation had to be loud enough to be heard.—I am, etc.,

Preston, Sept. 2nd. VINCENT M'NABB.

SIR,—I hope that the controversy into which you have entered with Dr. Mooney will be continued by other disputants, and that the medical profession will consider seriously the position which will be created by the Mental Deficiency Bill shou'd it ever happen to become an Act.

In your leader you state that this bill "has for the most part met with the approval of those who are interested in the social welfare of the people." I am afraid that these distinguished persons do not bring their approval very forcibly before the public; for though while in Paris last May I happened by chance to read of the introduction of this bill, since my return to London I have met hardly any ordinary individual who has heard of it, much less learnt anything of its provisions.

It appears to me that your ready acceptance of this bill is akin to the error that has been perpetrated in so much of recent legislation—namely, a desire to take a short cut to the millennium. You say that "the eugenists are to-day mainly searching for fuller knowledge"; and everybody would be willing that they should do so in a reasonable manner. But if that search means an experiment by Act of Parliament before the eugenists have sufficiently completed their investigations or adequately formulated and expounded their views, I venture to think that the *corpus vile* of the public is justified in resenting their attentions.

I sincerely trust that I am right in thinking that even a latter-day House of Commons will decline to accept the absurd definition of feeble-minded persons given in the bill; and I hope that the subject will be investigated and discussed for a much longer time before the medical profession is called on to decide whether a person is capable of competing on equal terms with his fellows.—I am, etc.,

London, W., Sept. 3rd. CHAS. BUTTAR.

FORCIBLE FEEDING OF SUFFRAGE PRISONERS.

SIR,—I strongly protest, as I did upon a former occasion, against Sir Victor Horsley and others using the JOURNAL to exploit their political views. Whatever reasons may be adduced as to the expediency in publishing preliminary reports, the statement, "Full details of what steps were sometimes taken by the officials in different prisons cannot be ascertained," conclusively proves this preliminary report to be chiefly founded upon *ex parte* evidence and rushed into print with the intention of creating party political animus, certainly contrary to the better interests of the JOURNAL, its motto being, "*Audi alteram partem.*"

Interesting as the results of the forcible feeding of suffrage prisoners may be, neither the statements of a Minister of the Crown nor the reasons which induced suffrage prisoners to refuse their food are topics for a purely non-political journal.—I am, etc.,

Bletchley, Sept. 1st. P. BROOME GILES.

Sir,—I am sure that all who have read the "report" on pp. 505–8 in the BRITISH MEDICAL JOURNAL of August 31st must feel greatly surprised and exceedingly pained that such medical men as Mr. Moullin and Sir Victor Horsley could concoct and authorize a paper containing such grave charges against brother practitioners, and such a partisan summary of the statements of prisoners of that kind. As long as the law exists which declares suicidal attempts a crime, so long must we agree that forcible feeding is emphatically necessary for people who adopt starving, instead of a "bare bodkin," to threaten their existence, if

not to take their quietus; and it is certainly as necessary and ordinary a treatment to preserve their health as any other means with irresponsible people. Whatever discomfort it may cause, it is not dangerous, and Mr. Ellis Griffith was perfectly right in saying, "If pain was suffered, it was *entirely* due to the resistance offered to what was necessary treatment." The "physical and mental torture" was entirely due to the subjective feelings of these women, who were all abnormal excitable individuals, with mental instability. Their conduct and "statements" show this clearly and unmistakably to any one. All the "facts" and "written statements" of such people must be largely discounted, and I, for one, do not believe them, especially the absurd and vindictive "statement" that a wardress used a cup with a broken edge, put so as to imply, with a *suggestio falsi* that in one case such a cup was sawed along the gums. It happens opportunely that some apropos remarks from an expert, Dr. M. S. Wood, appear on p. 671 of the *Lancet* of August 31st, which every one will endorse:

> Refusal of food is generally an indication of severe mental disturbance. . . . When a patient, from whatever cause, refuses food it is unwise to delay forcible feeding, as ample diet in itself constitutes a valuable therapeutic measure . . . The passage of a soft tube causes no real pain nor injury.

As to the unseemly, unprofessional, unfounded, and entirely uncalled-for reflections, made with such an arrogant assumption of superiority, on the skill and judgement of the medical officers concerned, *whose mouths are sealed*, the nature of them may be shown by one charge, namely, "preventing for three or four hours access to the lavatory," when, as a matter of fact, utensils for this are in every cell; and it will strike all who have had to deal with such a class of patient, that the greatest praise must be given to the medical officers and all concerned, that, in the carrying out of most unpleasant duties towards such refractory, violent, and irrational prisoners, no more untoward results have followed; and Mr. McKenna deserves the best thanks of the community in resisting senseless, sentimental, class clamour; and in maintaining the discipline of places which are, after all, for equality of treatment for evildoers, not theatres for rehearsals by "normal-minded women" of insane acts, and "playing to the gallery" in so very lamentable and poor a way. The only good to be hoped for from such a (in every respect regrettable) report is, that suffragettes seeing it may cease to do evil and learn to do well.—I am, etc.,

Winchester, Sept. 1st. WILLIAM MORTON HARMAN.

SIR,—In the "Report on the Forcible Feeding of Suffrage Prisoners," published in the JOURNAL of August 31st, such a lurid picture is drawn of the horrible effects of this method of administering nourishment that many of us who have been in the habit of adopting it daily in our practice are appalled with the thought of the devastation which we must have caused! Yet when we consider the report maturely our spirits revive once more. We understand that it is the handiwork of devotees of the Zolaesque school, whose habit is to drag in every unpleasant detail in order to heighten effect, excluding everything that tends to diminish the sombre hue of tragedy which they are desirous of obtaining. Working on these lines, what a heart-rending description one might give of the passing of the catheter or of the opening of an abscess! It is indeed curious to reflect, too, how little observant medical men are of the dreadful results which their treatment appears to bring about. For many years the tube has been passed in order to introduce food into the stomach and to wash out the stomach in cases of dyspepsia or of poisoning; yet these dire effects have been overlooked. When, however, a certain number of members of society contravene the laws of the community, and for the protection of the community and its property are haled off to prison, results follow this process of introducing the tube out of all proportion to those which have been observed previously.

It may be frankly admitted that occasionally a patient will, after being fed by the nasal or by the œsophageal tube, complain of irritation of the passages; or, again, there may be sometimes a temporary spasm of the glottis, especially on the earlier occasions when feeding takes place which may necessitate withdrawal of the tube and discontinuance of the process for the time. The latter condition is, however, rare. Retching and even vomiting are of course to be looked for in certain cases, especially in those with sensitive fauces, but this seldom gives rise to any danger if the patient's head is turned to one side. If the tube is well oiled—and particular care should be exercised in this respect—it is difficult to realize why there should be such profound irritation of the passages as the writers of the report describe; and when one considers the strain put upon the fauces of the average person by hot, bulky, or imperfectly masticated boluses which they swallow, tube feeding may quite properly be looked upon as more likely to be restful to the throat than otherwise.

The whole "report"—though it is not so much a report as a carefully planned piece of special pleading—is unconvincing to any one who is practically acquainted with forcible feeding. To such practical acquaintance the compilers lay no claim. It is likewise as marked an example of the *post hoc* reasoning as one could wish not to see; and it seems that if women after forcible feeding suffer from any complaint, from "pruritus" to "nightmares," *ergo* it is the feeding that has done it.

You closed your columns, sir, to the discussion of this matter after a previous pontifical utterance by the same writers. I trust that now you will allow that discussion to take place, in order that an opportunity may be given of traversing further this amazing "report."—I am, etc.,

London, S.E., Sept. 2nd. HUBERT J. NORMAN.

SIR,—Having read in this week's BRITISH MEDICAL JOURNAL the account of the forcible feeding of the suffrage prisoners, I am surprised that medical men can be found to carry out such operations, which I consider to be degrading them to the level of common executioners. Not that I sympathize with the foolishness and misguided tactics of those who resort to such subterfuges as hunger strikes to escape justice. All this forcible feeding, however, and the violence necessary to enforce it, defeat their own ends, as the victim is thereby enabled to pose as a martyr. But why all this pother? Why not let the prisoners starve themselves if they wish to do so? A few days', or a week's, starvation would do them no harm, or at least less harm than forcible feeding; and directly it appeared, in the judgement of the medical officer, to be endangering life, they could be released on the understanding that immediately they appeared in public, before the expiration of their sentence, they would be re-arrested, when they could repeat the process of starvation if they felt disposed. In the meantime, on their discharge their friends would take them in hand and doubtless feed them up, but it would be probably some time before they would be able to renew any aggressive action, and they would be obliged to complete their term of imprisonment, if not in gaol, at all events in their own homes, where they would be a charge on their friends, and thus save the expense and trouble of having them in prison.—I am, etc.,

Bentham, September 2nd. ROBERT BRADLEY.

SIR,—While filling our JOURNAL with harassing details of the results of suffragists struggling with their warders, would it not be well to point out in general terms what should be done with a prisoner who refuses food?—I am, etc.,

Felsted, Essex, Sept. 3rd. DONALD SMALLPEICE.

SIR,—With regard to the article on forcible feeding written by three sympathizers with women suffrage, which is published in the JOURNAL of August 31st, I desire to point out that we require more impartial evidence than that which is contained in the "report" before we can credit the very grave charges made against the medical officers of His Majesty's prisons.

The JOURNAL is not a political organ, and therefore a discussion of the question is out of place in its columns. The treatment of the suffragists is a question between them and the Government, and it is one with which the medical profession has nothing whatever to do, and unproved charges of "malpractices and torture" should not be made in the JOURNAL of the Association against doctors who are doing their duty, which in the case of the suffragists must be a very unpleasant one.

If a case of malpraxis can be made out against the prison medical officers, the suffragists have their remedy,

but it is obvious that the BRITISH MEDICAL JOURNAL is not a suitable organ for the ventilation of suffragist grievances.—I am, etc.,

London, Sept. 3rd. A. G. WELSFORD.

SIR,—In the lengthy and interesting preliminary report on the forcible feeding of suffrage prisoners, by Dr. Savill, Mr. Mansell Moullin, and Sir Victor Horsley, published in your issue of August 31st, there are certain statements made and opinions advanced that invite examination and discussion.

Under the heading "Effects on the Nervous System" the authors depart from the clinical nature of the previous paragraphs, and make various statements of a more general nature, of which I select the following in the hope that there may be some interchange of views regarding the correctness of the opinions they indicate.

"These women are normal individuals who go to prison as political offenders." Are they normal individuals? Are the actions, for which they have been committed to prison, those of normal women? Is it the case that they go to prison as political offenders?

"We merely point out that on admission the prisoners are in a normal mental condition." Do the authors mean the normal mental condition of suffrage prisoners, or the normal mental condition of the average British woman?—I am, etc.,

Inverness, Sept. 3rd. T. C. MACKENZIE.

SIR,—I have read the preliminary report of Dr. Agnes Savill, Mr. Mansell Moullin, and Sir Victor Horsley in your issue of August 31st, and would be glad of your courtesy in allowing me to present an aspect of the case that seems to me of paramount importance, as affecting the honour of the medical profession.

Three consultants, after due investigation, have stated certain facts bearing on the medical treatment of prisoners in H.M. Prisons, which facts have not up to the present been denied. Further, it appears that this medical treatment has been administered by medical colleagues acting under orders with a two-fold purpose: (1) To prevent starvation; (2) to prevent these prisoners from terminating their sentences, and in most cases, after a medical examination, to ascertain, not whether the prisoner is too ill to take food naturally, but whether she is strong enough to stand the ordeal. From medical and other evidence, the physical condition of the prisoners so fed (either with or without resistance) is considerably worse than before feeding—for example, sepsis of nose, throat and mouth, lung conditions, dyspepsia from large, cold quantities of food introduced into contracted stomach, etc.

Obviously, then, forcible feeding in these cases has not prevented starvation, or at least cannot be said to have been administered with the result of improving the physical condition of the patient who is unwilling or unable to take food, for it has actually reduced her to a condition of collapse, rendering further forcible feeding, or, indeed, detention in prison, unsafe. Further, forcible feeding has the result (unintentional, of course), of terminating the sentence prematurely.

It seems, then, that the twofold purpose, (1) to prevent starvation, (2) to prevent termination of sentence, is not fulfilled, and I cannot see the *raison d'être* of prison doctors continuing to administer the treatment, unless, indeed, they are called upon by the special nature of their appointments to administer treatment that can only be regarded as punishment, in the shape of physical and mental torture.

If this is so, the time is surely ripe for the medical profession, through its organization, to protest against the imposition on its members of duties that are distinctly unprofessional, for it is probably difficult, if not impossible, for any individual medical officer to refuse to carry out the instructions of his authority. I fail, however, to recall in this connexion any other branch of the medical service where the necessity for the imposition of any form of medical treatment does not lie solely with the medical officer, whose decision is final. Apparently in the case under consideration, the prison doctors take their orders for this special form of medical treatment from their lay authority, which is surely an abrogation of their privilege to prescribe or withhold treatment unbiassed by any consideration, except the need of health.

Are prison doctors called upon to render purely medical services, and are they in order, as members of the medical profession, in administering a form of treatment to the patients under their care which rapidly reduces these to a serious condition of invalidity? Even in Russia, during the savage flogging of prisoners, the prison doctor stands by and raises his hand when in his opinion the prisoner has had enough. Here, apparently, the prison doctor administers the torture himself, and then decides from the prisoner's physical condition when she has had enough of his treatment.

I have before me a copy of the oath taken by Arabian doctors at the Kasr-el-Ainy Hospital at Cairo, which begins and ends:

> I swear in the name of God, the Most High, and of His Sublime Prophet Mohammed, whose Glory may God increase, to be faithful to the laws of honour, honesty, and benevolence in the practice of medicine.
>
> * * * * * * *
>
> May I be respected if I remain faithful to my vow. If not, may I be covered with shame and be despised. God is my witness to what I have said. The oath is finished.

Surely the doctors of the West will not yield to their colleagues of the East in high aim and purity of motive!—I am, etc.,

Harrow-on-the-Hill, Sept. 10th. BARBARA TCHAYKOVSKY, M.D.

TUBERCULIN TREATMENT.

SIR,—In my letter of August 17th in answer to Dr. Prest I dealt with his statement of August 10th. Referring to tuberculin administration, Dr. Prest says:

> The chief desideratum at present would seem to be to reconcile the conflicting schools of thought on this subject.

He instanced Dr. Nathan Raw's work and views and my views; and I merely showed how these conflicting views could be explained. In his last letter Dr. Prest merely quotes the opinions of Dr. Arthur Latham and the work of Drs. G. B. Hawes and Cleveland Floyd.

These views are further evidence as to the value of tuberculin *in small doses*. It is too obvious that in the cases of Drs. Hawes and Floyd a similarity of results without or with tuberculin (used in small doses) is to be expected, because the small doses have little or no permanent effect. In America, at the Phipps Tuberculosis Dispensary (see BRITISH MEDICAL JOURNAL, September 16th, 1911, page 635) the maximum dose is stated to be 0.005 gram T.R. Dr. H. Floyd gave a maximum dose of 0.2 up to 8 mg. of T.R. Even 8 mg. of T.R. can only mean 0.008 gram of T.R., which is quite too small to have any effect. According to my views tuberculin fails in small doses, and therefore the evidence of Drs. Hawes and Floyd merely supports me. On the other hand, large doses succeed, and I cannot discuss the subject with Dr. Prest till he brings forward evidence proving that tuberculin used in large doses has failed.

Further, he says:

> I was desirous of trying to find out whether it was safer in difficult cases to begin with relatively large or relatively small doses.

I may tell Dr. Prest that not only in difficult but in easy cases it is always right to begin with relatively small doses. Neither I nor any one else, so far as I know, has ever thought otherwise.

Then Dr. Prest says:

> The difference in the ideas of sanatorium physicians and doctors at dispensaries is probably due to the different type of case dealt with.

He seems to forget that a large proportion of the cases we are treating at the dispensary are but failures of sanatorium physicians. Dr. Prest is taking for granted the very point that needs proof, and my work proves that so long as the disease is tuberculosis, tuberculin treatment is the best. The *difference of type* he speaks of is probably due to a *different disease*, especially an intercurrent mixed infection.

Dr. Prest refers to Professor Sahli's book. I have read this book of 120 pages, 80 of which are devoted to a fanciful disquisition upon the action of tuberculin, and the remainder to the details in regard to the use of Beraneck's tuberculin. In the 120 pages there is not one single line of evidence to show that Beraneck's tuberculin is useful, and I would refer Dr. Prest to the report upon Beraneck's

tuberculin issued by the Gesundheitsamt of Berlin. We have all to learn about tuberculin treatment and its action. Perhaps Dr Prest has to learn as much as any of us.

Facts are more valuable as evidence than fancies. I will give two facts:

1. A doctor long associated with sanatoriums lost his health. He was advised by several physicians to go to the seaside for change of air. He went, and returned worse rather than better. He was then examined by several physicians, and active trouble was discovered, with tubercle bacilli in the sputum. He was advised to have sanatorium treatment; he did so, and was worse after the treatment. He left the sanatorium and went home. In despair he came to me, and I found evidence of active disease in the third stage, in spite of sanatorium treatment when the disease was early. He has since had tuberculin treatment, and admits himself that tuberculin treatment has done him more good than any other. How does Dr. Prest classify this type?

2. A young girl with active disease was sent by a governor of Mount Vernon Hospital to this hospital for treatment. In spite of the governor's interest this girl was refused treatment, and again in despair the governor applied to me. I treated this active case (with tubercle bacilli in the sputum—rejected by the Mount Vernon Hospital) with tuberculin alone. The disease is arrested, tuberle bacilli have disappeared, and the governor took the trouble to send the sputum to the Mount Vernon Hospital for examination on three different occasions, and always received the answer, "No tubercle bacilli present." Will Dr. Prest also classify this type?

I merely mention these cases because a doctor was the victim in one instance, and the governor of Mount Vernon Hospital can vouch for the truth of my version in the second instance.

Lastly, will Dr. Prest kindly inform me what are the biggest doses of tuberculin he has used at one dose, and the form of the preparation? We may then know where we are in the discussion.—I am, etc.,

W. CAMAC WILKINSON, M.D., F.R.C.P.,
September 3rd. Director Tuberculin Dispensary.

THE CAMPAIGN AGAINST TUBERCULOSIS.

Sir,—It is of the utmost importance that the national campaign against tuberculosis should be launched on right lines. To use the machinery of the office of the local medical officer of health may be the easiest course, but it is not the best one.

Agreed, consumption is an infectious disease, but it cannot, therefore, be dealt with in exactly the same way as other notifiable diseases; and the medical officer of health, already overworked, is not of necessity an expert on diseases of the chest.

What is required is a whole-time tuberculosis officer, who is a recognized expert, to act as consultant in friendly co-operation with the local family practitioner in the combat with this scourge.

That, clearly, is the proper line of attack, and not the relegation of the whole matter to a department, which—important ally though it be in improving the public health—is, in the matter of tuberculosis, in many instances, out of touch with the actual treatment of patients.

Medical officers of health may be experts in diagnosing infectious diseases in the ordinary acceptance of that term; but the fact that tuberculosis has recently, and rightly, been included in the list by official decree does not endow this important service with a special training in diseases of the chest. They cannot become specialists in a moment.

Efficiency in this matter can only be attained by separating the duties of the tuberculosis officers from those of the medical officers of health.—I am, etc.,

J. WEBSTER WATTS,
Manchester, Aug. 30th. Secretary, National Medical Union.

OPERATIONS FOR GLAUCOMA.

SIR,—In reply to Major Elliot's letter I have to point out that we use the word "home" in different senses. When I indicated that Major Elliot went "home," I meant that he returned to the place where he ordinarily resides—namely, India. Major Elliot uses the word "home" in a restricted and conventional sense, and by "home" he means the United Kingdom. There is, of course, no objection to such a use of the word provided the convention is clearly understood or definitely stated. This point being made clear, I have now to call attention to the fact that my first operation by simple trephining was done on February 5th, 1909. That I was doing trephining operations was perfectly well known both in Edinburgh and in London by April of that year. Major Elliot returned to Madras in June, 1909, and did his first operation on August 2nd, 1909. So far as I am aware, Major Elliot's first communication to the *Ophthalmoscope* was in December, 1909, or nearly a year after I had done my first simple trephining operations.

I have obtained excellent and permanently good results by simple trephining. Some I have now had under observation from 1909, and they are still good, but on one or two occasions the success was not all that I could desire, and so for certain patients I have now altered the method of procedure by combining with the trephine operation a cyclodialysis.

Some day or other I may let such ophthalmic surgeons as are interested and as are possessed of open minds, and who do not shut themselves up rigidly to one form of operation, know why I sometimes make this addition. I feel sure that I am not in error in thinking that operative methods should be altered to suit special requirements.

Now, as regards publications in the *Ophthalmoscope*, I regret that Major Elliot has not seen his way to accept my statement that I do not see the *Ophthalmoscope*. I would not have made that statement were it not perfectly true. On one occasion one of my assistants said to me that a contributor to that journal had published an account of an operation for glaucoma which operation was the one that I had been doing as occasion required since February, 1909. I read the article to which he called my attention, but, so far as I remember, I have not had the advantage of seeing any other of Major Elliot's communications to the *Ophthalmoscope*.

Lastly, as to trephining in the neighbourhood of the ciliary body. Not many years ago all junior surgeons were warned by their seniors as to the extreme, almost fatal, danger of wounding the peritoneum in abdominal surgery. Such fears have been shown to be absolutely groundless. Similarly, a wound in the ciliary body of a reasonable amount is not followed by serious consequences if it be made absolutely aseptically.—I am, etc.,

Glasgow, Sept. 5th. FREELAND FERGUS.

MEDICAL MISSIONS, S. P. G.

SIR,—Can any of your readers kindly help us by suggesting a suitable man to be travelling secretary of the Medical Missions Department of the S. P. G.—*either* to undertake office work and devote a certain proportion of his time to speaking at meetings, etc., *or* to give practically his whole time to attending missionary exhibitions and doing deputational work? In either case the post would offer a splendid opportunity for useful service; and in either case the ideal man would be a doctor in holy orders, who has had some experience of medical mission work abroad and is not too old to adapt himself to new circumstances. It is not a *sine quâ non* that our new secretary should be possessed of all these qualifications, but it is essential that he should be very keen about missions, and an earnest, spiritually-minded man. He must also be a good speaker and organizer, and in the former case, at any rate, have some business capacity. For further information please see advertisement, p. 49.

We are greatly in need of a competent helper in the medical ward at missionary exhibitions—some one who is capable of giving interesting demonstrations or practical addresses on medical missions. Until we succeed in getting a secretary who can undertake this work or a permanent deputation, we should be grateful for temporary assistance at exhibitions.

The Medical Missions Fund of the S. P. G. is quite distinct from the General Fund of the society, and is administered by a special committee largely composed of medical men and women. It is steadily growing, but at present it is utterly inadequate to meet the urgent appeals for medical help constantly received from abroad.—I am, etc.,

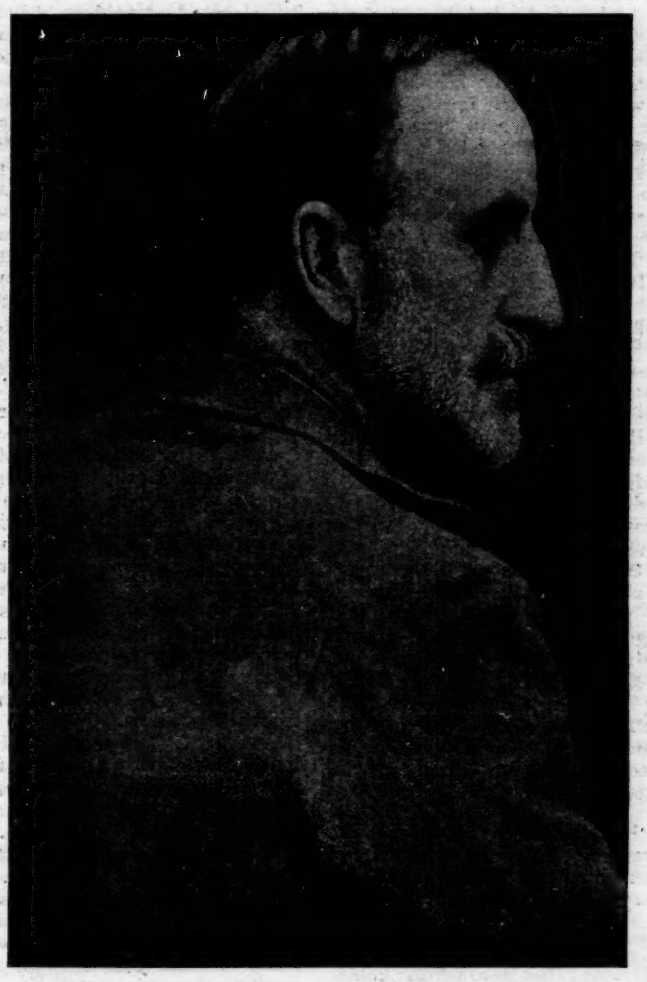
E. H. MOSSE,
Chairman, Medical Missions Committee, S. P. G.
St. Paul's Rectory, Covent Garden, W.C.,
Sept. 10th.

Obituary.

CLINTON THOMAS DENT, M.A., M.C.CANTAB., F.R.C.S.,

SENIOR SURGEON, ST. GEORGE'S HOSPITAL; VICE-PRESIDENT OF THE ROYAL COLLEGE OF SURGEONS; CHIEF SURGEON TO THE METROPOLITAN POLICE.

To a wide circle of friends the news of the death of Clinton Dent, on August 26th, must have come as a painful shock, for he was apparently in his usual health when he began his summer holiday, and his fatal illness—a virulent pure septicaemia, possibly due to oral sepsis—lasted little more than a fortnight. He was the eighth child and fifth son of Thomas Dent, and was born on December 7th, 1850, at Sandgate. He was educated at Eton and at Trinity College, Cambridge, where he took the ordinary B.A. degree in 1873. Subsequently he examined in surgery for many years, and was made Hon. M.C. in 1899. He entered the Medical School of St. George's Hospital, and after holding the usual appointments was elected Assistant Surgeon in 1880, and for a time was Joint Lecturer on Physiology in the Medical School. In 1895 he became full Surgeon, and at the time of his death was Senior Surgeon and Chairman of the Medical School Committee. He was also for many years Surgeon to the Belgrave Hospital for Children, which owed much to his constant guidance and generosity. In 1904 he undertook the congenial duties of Chie Surgeon to the Metropolitan Police, thus serving in the same capacity as his former teacher and colleague, the late Timothy Holmes.



Photograph by] CLINTON THOMAS DENT. *[Elliott and Fry.*

Ample private means, which sometimes interfere with professional activity, had no such paralysing influence on Dent, and merely enabled him to concentrate his energies on worthy objects. He took an active part in the medical societies of London; he served as Secretary (1901–4) to the Royal Medical and Chirurgical Society, before which he read papers on "Four Hundred Cases of Amputation" (1890, with W. C. Bull), "The Behaviour of a Tendon Ligature" (1891, with S. Delépine), "Amputation of the Entire Upper Extremity for Recurrent Carcinoma" (1898), "Congenital Hypertrophic Stenosis of the Pylorus and its Treatment by Pyloroplasty" (1903, with E. Cautley), and at the time of his death was President of the Surgical Section of the Royal Society of Medicine. He was also Secretary and Vice-President of the Medical Society of London, and in 1908 delivered the annual oration on "The After-Results of Injuries," in which he embodied some of the experiences derived from work among the Metropolitan Police in connexion with the difficult subject of traumatic neurasthenia. As Hunterian Lecturer at the Royal College of Surgeons in 1905 he dealt with "Congenital Hypertrophic Stenosis of the Pylorus" and on "The Forms of Softening of Fracture of the Bones occurring in Childhood and Youth." It is to be regretted that these lectures were never published, for he had paid much attention to both of these subjects, and in the second lecture collected a number of original observations, and brought forward evidence to show that mollities ossium might be a sequel of infective disease. He was a most fastidious and critical writer, and has left behind a number of original and interesting notes and lectures in manuscript. For some years he was lecturer to the nurses of St. George's Hospital. These lectures, which were fully written out, he sometimes spoke of publishing, but this was never accomplished. He had long been interested in John Hunter, and in 1901 published a beautifully got up pamphlet as an explanatory notice of the picture of "John Hunter leaves St. George's Hospital, October 16th, 1793," by A. D. McCormick, R.B.A. It is sad that the material he collected will never see the light in any Hunterian Lecture of his; some of these data he generously gave to a previous orator, but this by no means exhausted his store.

At the Royal College of Surgeons, of which he became a Fellow in 1877, he was a member of the Court of Examiners (1902–1911), had been on the Council since 1903, and at the time of his death was senior Vice-President. His friends cannot but feel disappointed that his life has been cut short before it was crowned by the honour of the Presidency, for which he was so manifestly suited by his scholarly knowledge of surgery, his striking personality, and his exceptional powers of oratory. The latter was remarkably shown by his delivery without a note of the introductory address to St. George's Hospital twenty-five years ago on "The Nature and Significance of Pain," which was afterwards printed.

In December, 1899, he went out on his own initiative to the South African war, where he acted as correspondent to this JOURNAL. On his return he delivered an address on "The Wounded in the Transvaal War" before the Royal Medical and Chirurgical Society.

As a surgeon he was conscientious, careful, and at times, especially in the face of unexpected difficulties, extremely brilliant. He was not inclined to operate unless he was convinced of the need and that benefit would result; and he was always anxious to make a diagnosis before rather than at the operation. In spite of his critical attitude to his own work, his professional writings covered a wide field, for, in addition to those already mentioned, he translated and edited *Billroth's Clinical Surgery* for the New Sydenham Society (1881), contributed various articles to Heath's *Dictionary of Practical Surgery*, wrote on "Traumatism and Insanity" in Tuke's *Dictionary of Psychological Medicine* (1892, vol. ii), "The Development of London Hospitals during the Nineteenth Century" (*Lancet*, 1898, ii), "Insanity and Surgical Operations" in Allbutt's *System of Medicine*, "The Surgery of the Heart" in Musser and Kelly's *System of Treatment* (1911), and "Intestinal Obstruction" in Latham and English's *System of Treatment* (1912, vol. ii). His wide acquaintance with various forms of athletics prompted him to write an

interesting article on "Periostitis following Muscular Exertion" (*Practitioner*, 1897).

Between 1904 and 1911 death or premature retirement deprived the active staff of St. George's Hospital of four on the surgical and three on the medical side, and at the moment it is difficult to realize all that this further loss entails. Dent was a staunch and generous friend to the school, an ideal and absolutely impartial chairman of committees, a wise leader, and a charming colleague. Though the time limit would have necessitated his retirement from the staff in 1915, he was fully in sympathy with young men, and thus was a successful and graphic teacher.

As an Alpine climber Mr. Dent had so long been famous that the world at large thought of him in this, rather than in his professional, capacity. He made the first ascent of the Aiguille du Dru after eighteen unsuccessful attempts, the first ascent of the Rothhorn from Zermatt, and of other Alpine peaks, but his greatest achievements were in the Caucasus, where he not only climbed the peaks, but explored the range. He undertook in 1889 the sad duty of searching for the bodies of W. Donkin and H. Fox, who were killed on Koshtantau, in 1888; he had gone out with them and would have shared their fate had it not been that he was prostrated by illness. He wrote a charming book, *Above the Snow Line*, published by Longmans in 1885, edited and wrote a large part of the volume on *Mountaineering* in the Badminton Series (1892, third edition 1900), contributed largely to *The Alpine Journal*, and gave a Friday evening lecture at the Royal Institution "On the Influence of Science on Mountaineering" (1895). He also gave two juvenile lectures on "How Mountains are Made and Destroyed" before the Society of Arts in 1897. He joined the Alpine Club in 1872, was elected to the Committee in 1874, was Secretary (1878-1880), Vice-President (1884), and President (1887). It is curious that Sir Alfred Wills, who was the third President of the Club (1863–1865), also entered into the valley of shadows early in August. Dent was the first and only President of the recently formed Association of British Members of the Swiss Alpine Club. He was an expert photographer, and often exhibited the fine effects which he had obtained in the Alps and Caucasus, especially at the Graphic Society of St. George's Hospital, of which he was at one time President. He had an extensive collection of photographs of patients, and last year gave a cinematograph demonstration of gastric peristalsis in hypertrophic stenosis of the pylorus before the Section for Diseases of Children of the Royal Society of Medicine. He had wide interests, was a member of the executive committee of the Athenaeum, belonged to many social clubs, and leaves numerous friends. Earlier in life he was fond of acting, wrote some farces, and under the title, *Fruit and Blossom*, adapted Pailleron's *Etincelle*.

H. D. R.

The following tribute was circulated throughout the Metropolitan Police District on August 27th by the Commissioner, Sir Edward Henry, G.C.V.O., C.S.I.:

> It is with deep regret that the Commissioner acquaints the Force of the death of the chief surgeon last evening.
>
> A singularly able man, he devoted to the Metropolitan Police, from the time of his appointment in 1904, his whole-hearted efforts. The Police Medical Service has been greatly improved under his care and guidance, and those who have been brought in touch with him by sickness will long remember the personal and kindly interest he took in every case. The Commissioner feels that he has lost an able and fearless counsellor in all medical questions affecting the wellbeing of the Force.
>
> (Signed) E. R. HENRY.

The memorial service, held on Friday, August 30th, at St. Peter's, Vere Street, was largely attended by friends and by representatives of the Royal College of Surgeons (Sir Rickman Godlee), the Royal Society of Medicine (Sir H. Morris), the Metropolitan Police, the Alpine Club, St. George's and the Belgrave Hospitals; and the burial at Kensal Green Cemetery was followed by hundreds of policemen who voluntarily came to show their respect.

By order of the Commissioner of Police, the body was carried into the church at Vere Street by men of the C Division, and at the cemetery it was carried to the grave by men of the X Division.

AN OLD COLLEAGUE writes:

In the death of Clinton Dent the profession deeply mourns the loss of a great personality—a personality which made itself felt in all that he said and in all that he did. Throughout the whole of his very active life Dent was always "up and doing." He was a man of great force of character, of wide and comprehensive views, absolutely unbiassed by petty jealousy or party spirit, both in his professional work and that—and it was great—which lay outside it. A voracious reader, he had acquired an extensive and accurate knowledge of many subjects connected and unconnected with his profession as a surgeon. All that he read he inwardly digested; in all that he did he was earnest, thorough, and scrupulously conscientious. To employ the expression "a right judgement in all things" may be strong language in the case of any human being, but it may be truthfully averred that in a delicate or difficult question the greatest respect was invariably paid to Dent's judgement. His opinion never failed to carry weight, because it was universally accepted that he never spoke at random or expressed an opinion without careful consideration of the point in debate.

Such attributes made him an ever helpful and valuable colleague in his various fields of action, whether it were as Surgeon and Governor of his hospital, in his office in Scotland Yard as Chief Surgeon of Metropolitan Police, on the committee of the Alpine Club, or in the less important associations with which he was connected.

He combined in a marked degree both theory and practice. He was an enthusiast in everything that he undertook, whether it was work or whether it was play. To his superabundance of energy and activity of thought it was due that in lecturing, and even in ordinary conversation, his words at times followed one another with so much rapidity that it was difficult for a listener to grasp his meaning and occasionally to distinguish his words. But in his prepared speeches or addresses it was a real pleasure to hear him. They were always good. None of those who heard his introductory address at the opening of the session at St. George's Hospital on "The Nature and Significance of Pain" (delivered without a note of any kind) are likely to forget the impression that it made.

It was not all mere brain work with Dent. He was a man of many accomplishments. As a photographer he was unsurpassed; as a mountaineer he had few equals. His pictures of the High Alps and the Caucasus show him to have been a true artist; they formed an interesting and pleasing group in many an exhibition. His professional attainments and his contributions to literature and science have already been set forth and may well be omitted from this memoir.

As an after-dinner speaker Dent was peculiarly attractive by reason of his manner and his matter. The speeches he made sparkled with wit and humour; otherwise they gratified the more serious members of his audience.

As a host he had few peers. One of his great delights was to entertain a few friends at his dinner-table; most pleasant dinners they were. It was on such occasions that he displayed his knowledge of a vast variety of subjects, and it was realized by his guests to what an extent he made himself master of them. His house was filled with art treasures—ample evidence of his good taste in silver plate, furniture, pictures, embroidery, and what not.

Dent was in all respects a well-read man, and the results of his reading in conjunction with his originality illumined his conversation. But it was during the later years of his life that what may be termed his chief work was done—from the time, that is to say, when he was appointed Chief Surgeon to the Metropolitan Police. From the date of that appointment in 1904 his heart and soul were in his work in Scotland Yard, and most excellent work he did there. This office is no sinecure, but to Dent, so far as one could judge, it was almost child's play, so inexhaustible was his energy, so great his ability to solve a difficulty. He rendered yeoman service to one of the most important departments of municipal London, and his name will be intimately associated with many recent improvements in the organization of the Force. He was popular with the divisional surgeons, and was liked by the men.

With his evenly balanced mind, he avoided mistakes arising from hasty conclusions. He was shrewd in matters of business, but scrupulously honourable in his dealings

with men. As a consequence, he secured to himself the utmost confidence in his integrity of all with whom he came into contact. He was a clever, able, upright man, and he was a staunch friend. Echo answers *Qualis erat!*

SIR WILLIAM SINCLAIR, M.D., M.R.C.P.,

PROFESSOR OF OBSTETRICS AND GYNAECOLOGY, VICTORIA UNIVERSITY, MANCHESTER.

WE regret to announce the death of Sir William Sinclair, Professor of Obstetrics and Gynaecology in the Victoria University of Manchester, which took place at Manchester on August 21st.

William Japp Sinclair, the son of Alexander Sinclair, was born at Laurencekirk, Kincardineshire, on March 6th, 1846. He was educated at the public school, Laurencekirk, at the Aberdeen Grammar School, and at the Aberdeen University. He took the M.A. degree with honours in natural science in 1869, and graduated (M.B., C.M.) with honours in 1873. In the latter year he was appointed Resident Medical Officer at St. Mary's Hospital, Manchester. Subsequently he was House-Surgeon at the Manchester Clinical Hospital for Women and Children for two years. In 1875 he went to Vienna to study obstetrics and gynaecology; took his M.D. degree at Aberdeen, and was appointed Honorary Physician to the Manchester Southern Hospital for Women and Children.

For many years after the death of Professor John Thorburn (1886), Sinclair, the only consultant on its staff, was in charge of the women's work of the Southern Hospital. He was its professional mainstay, and contributed very largely towards its successful work, with its somewhat limited accommodation and resources. He was instrumental in arranging for the regular instruction of monthly nurses and midwives in the small maternity department, and the home patient district of the hospital. Whilst Professor he did a certain amount of clinical teaching there, but the hospital was not large enough for certification, and most of the students attended the larger, and much older rival institution, St. Mary's Hospital. When the Southern and St. Mary's Hospitals amalgamated, Sinclair retained his post as Honorary Physician to the St. Mary's Hospitals, as they became, until his death. It was mainly through his influence that the Board of Management of the Southern Hospital obtained from the Victoria University a gift of land, on which it was proposed to build their new hospital. This gift was made on the condition that beds should be reserved for the professor of midwifery and for the lecturer on diseases of children. Under the deed of amalgamation the university retained their rights to beds in the combined hospitals.

He was a very active worker and writer in the Eighties, and in 1884, as the result of meetings at his house, the *Medical Chronicle* was founded, with himself and Dr. James Niven as editors. In 1887 he contributed to it a series of papers on "Gonorrhoea in Women," afterwards published, with additions, in book form (1888). These papers brought him into prominence, abroad as well as at home. He was thoroughly conversant with what was going on in the Continental clinics, which he visited a good deal. When the late Dr. Cullingworth resigned the Chair of Obstetrics and Gynaecology in Owens College to take up work at St. Thomas's Hospital, London, Sinclair was appointed his successor. In his application for the post he had many testimonials from leading gynaecologists abroad. With the growth of the college into one member of the federal Victoria University, and finally into the independent body it now is, he became university professor and retained the chair until his death, although owing to rather prolonged ill health the performance of his duties during the last two years manifestly caused him considerable physical distress.

Sinclair, especially as Professor of Obstetrics and Gynaecology in the university, but also from his natural bent, played a very prominent part in the controversy over the proposed Midwives Bill in the Nineties. Like nearly all his consultant colleagues, he was in favour of the State legislating for the proper registration of midwives, believing that the result must ultimately be for the benefit of the nation. The question was argued in Lancashire and Cheshire with a good deal of warmth, and as the matter at issue was too acute to wait for weekly treatment in the medical journals, and because of its public interest, the correspondence columns of the daily press were freely used in its discussion.

Sinclair was naturally a fighter; he was an able letter writer and debater, with a talent for clear, precise, and logical thought, which he expressed in virile language. In the midwives controversy he bore the brunt of attack and defence for others of his side, though even his friends may not always have entirely approved of his controversial methods. The controversy is now a matter of history, and nothing more need be said about it. Whilst Sinclair was such a "bonny fighter" that he might have made even Alan Breck Stewart jealous had that doughty warrior taken to the pen, he was by heart one of the kindliest of men and a good companion, with ready wit and a sense of humour. He had a very deep sympathy for the poor, and most of his battles were in what he held to be their interests. With all this kindliness of nature and friendliness, he was somewhat injudicious in his criticism, either in the lecture-room or clinic, of the opinions and treatment of other medical men, and a little intolerant of the views of even his own consultant colleagues when they happened not to agree with his own. Nevertheless, especially in the Nineties, he had a large following of practitioners who had been taught their work by him. In his later years ill health prevented his keeping up and moving so fully with the progress of knowledge as, in his younger days, he had set such a worthy example in doing.

One memory of his teaching may be specially referred to, namely, the abuse of forceps in midwifery. On this matter he held strong views, which were expressed with characteristic vigour. This question formed the subject-matter of his address at the annual meeting of the Association at Montreal, 1897. The address was published in the JOURNAL of September 4th of that year (p. 589). When the British Medical Association met in Manchester (1902) Sinclair delivered the address on obstetrics and gynaecology. His subject was "Carcinoma in Women chiefly in its Clinical Aspect." The JOURNAL, in its leading article on the address, characterized it as being "a masterly review of a terrible subject." Sinclair, whilst fully recognizing the value of the work of pathologists and bacteriologists, felt that it had tended to throw careful clinical work into the shade without giving much that would make up for the loss of the information essential for treatment that clinicians could alone obtain. He pointed out the important part which the family doctor played in the early diagnosis of uterine cancer, and showed that his work was made more difficult because women believed that cancer was always painful from the beginning, and because they did not think much of unusual uterine haemorrhage about the time of the menopause. They therefore did not consult their family doctor as early as they should. The treatment of cancer of the uterus specially interested him, and he was a strong advocate of removal of the womb by the vaginal operation. The more radical operations then becoming popular with gynaecologists were, he considered, quite unjustifiable.

Early in the Nineties, Sinclair, with some others practising gynaecology in the North of England, felt that in the large towns there was sufficient material for a society devoted specially to their own subject. At his instigation a meeting of men from Manchester, Liverpool, Leeds, and Sheffield was held in Manchester, and a society formed which, as the North of England Obstetrical and Gynaecological Society, has had a most successful career, not only by doing good work, but by bringing specialists from the various towns into frequent contact. Sinclair, until the state of his health forbade it, was a regular attendant at its meetings and a frequent contributor to its debates.

As already stated, he was one of the founders of the *Medical Chronicle*, and edited it for some years, taking up co-editing duties again at a later date for a short period, with responsibility for the section on diseases of women. He maintained to the end a great interest in the journal, and had always a cheery word of encouragement for its present editor.

He was also the chief promoter of the *Journal of Obstetrics and Gynaecology of the British Empire*. The preliminary correspondence necessary to the formation of the committee of management was very extensive, and was

carried out almost entirely by Sinclair. A few years after successfully launching it he withdrew from a share in editorial responsibilities.

In his early days Sinclair wrote a good deal. The first of his two books on gonorrhoeal infection in women was to a large extent a reprint of papers which appeared in the *Medical Chronicle* in 1887. He did not profess to be in any sense a specialist in venereal diseases, but he wished to put before the reader the state of the question of gonorrhoeal infection from the purely gynaecological standpoint. He collected and annotated all the recent opinions of other clinicians of repute, and added his own experience. He laid special stress on the clinical work and the opinions of Noeggerath, much misunderstood in England, and, of course, on the importance of Neisser's researches on the gonococcus. He hoped to arouse the profession in England to active interest in a subject which he believed was too much neglected, though of vast social importance. In his opinion "gonorrhoeal infection in women give rise to a group of diseases which, by reason of their clinical interest and their social and moral consequences, surpass in importance every other class of affections which claim the attention of the gynaecologist." The book was a very forceful contribution to the gynaecological literature of the day. It treated its subject exhaustively and lucidly, and undoubtedly succeeded in its object of attracting attention, both at home and abroad, to the conditions he described.

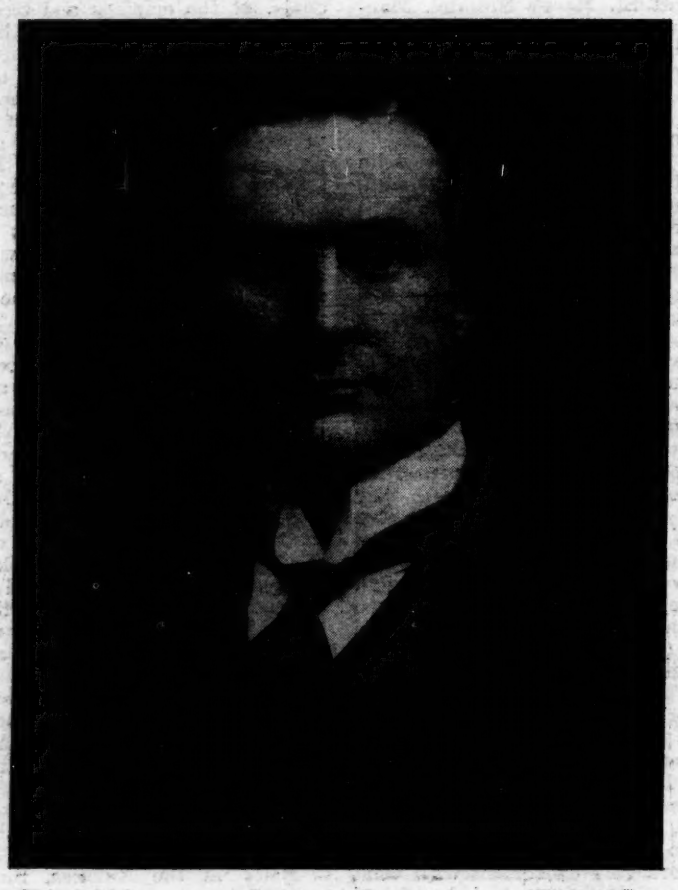
His only other work, on *Semmelweis: his Life and Times* (1909), was a very able account of the life of the man who realized that puerperal fever was a contagious disease, and of his difficulties and struggles in getting his idea accepted by his contemporaries.

He contributed the article on malignant disease of the uterus in Allbutt and Playfair's *System of Gynaecology*, and wrote many papers for medical journals.

Sinclair was President for a year of the Manchester Medical Society and of the North of England Obstetrical and Gynaecological Society, and he was a corresponding member of the Deutsche Gesellchaft für Gynäkologie and of the Royal Medical Society of Budapest. He was a member of the Central Midwives Board, and he acted for some years as Examiner in Midwifery and Medical Jurisprudence for Aberdeen University. He became a member of the Royal College of Physicians in 1892. He was also a J.P. for the City of Manchester.

Sinclair played a prominent part in the political life of Manchester after the days of the split over Home Rule, when he left the Liberal Party. He also was a member of the City Council for five years, representing the Conservative organization for one of the city wards. He was knighted in 1904. In 1883 he married Margaret, daughter of Andrew Haddon, of Denholm, Roxburghshire. Lady Sinclair and two daughters survive him.

E. MARGARET PHILLIPS, M.B. (Peppard Common) writes: May I offer a few words of appreciation of the late Sir William Sinclair? I admired him chiefly for two well-marked characteristics—his sense of justice and his tender-heartedness. I know that he was peculiarly sensitive about hurting his patients, and he made the avoidance of pain and suffering the chief theme of his teaching. I remember how forcibly he denounced stupidity in our profession, and particularly in his subject, as criminal. He was himself a most skilful operator, and probably few men have had the good fortune to save more lives than he has done. He was always kindness itself to me, and I sincerely mourn his loss.



Photograph by] LEONARD A. BIDWELL. *[Elliott and Fry.*

LEONARD A. BIDWELL, F.R.C.S.,

SURGEON, WEST LONDON HOSPITAL.

THE announcement of the death of Mr. Leonard A. Bidwell on September 2nd, following a serious operation, will have been received with the deepest regret by a very large circle of friends and colleagues—regret amounting to consternation in the minds of most, for few had any knowledge that his health was seriously impaired. Mr. Bidwell was the eldest son of the late Mr. Leonard Bidwell, Chief Clerk of the Post Office, and was born in 1865. He received his professional education at St. Thomas's Hospital, London, where he held the offices of Assistant House-Surgeon and House-Surgeon, and thereafter pursued a course of post-graduate study in Paris.

In 1891 Mr. Bidwell was elected Assistant Surgeon to the West London Hospital, becoming Surgeon in 1906, and Senior Surgeon in the present year.

From the commencement of his career as a surgeon his attention was drawn to the surgery of the abdomen, then in its infancy. During the marvellous development which has taken place in the past twenty years he was ever in the van of the progressive movement, and his sound judgement, courage, and exceptional technical skill and dexterity found wide recognition in an ever-widening circle of the medical profession and the public. Mr. Bidwell's class in intestinal and abdominal surgery in the Post-Graduate College was always filled, and often many students were waiting eagerly for the practical teaching in which he excelled, and in which he took the greatest interest and pleasure.

In May, 1896, two years after the foundation of the Post-Graduate College at the West London Hospital, Mr. Bidwell became Dean of the College and Hospital for teaching purposes, a position which he held to the last. Bringing to this work his characteristic energy, and an excellent capacity for business, he had the satisfaction of seeing a rapid growth in prosperity and usefulness of an institution which very distinctly supplied a want in medical education in London. In the first three years of its existence 50 graduate students sought instruction at the new school; during the last three years 671 students passed through, whilst the number of new entries reached the considerable total of over 2,500 during the period of his guidance. It is no exaggeration to state that the present-day prosperity and success of the college are largely due to his energy, originality, and perseverance.

Mr. Bidwell for many years was Surgeon to the Florence

Nightingale Hospital for Invalid Gentlewomen, and Consulting Surgeon to the Blackheath and Charlton Hospital, to both institutions devoting so large and generous amount of time in a very busy life that he will be sorely missed. He was also Consulting Surgeon to the City Dispensary.

He was not a prolific writer, but what he wrote was always practical and the outcome of personal observation and experience. His *Minor Surgery* was found of great service by practitioners and students for whom he was writing, and he was busy correcting the proofs of a second edition at the time of his death. His *Handbook of Intestinal Surgery* proved most acceptable and passed through two editions, whilst his numerous contributions to contemporary surgical literature were always marked by sound common sense and sincerity.

Mr. Bidwell took a great interest in the Volunteer Medical Service (now the Territorial Forces), serving for four years with an artillery corps and later with the Royal Bucks Hussars, in which he held the rank of Surgeon-Major at the time of his death.

In 1891 he became a member of the West London Medico-Chirurgical Society. He edited its *Proceedings* until the publication of the *Journal*, when he became Editorial Secretary. In 1905 he was elected President of the society, a position which he filled with dignity and with pleasure to the members and satisfaction to himself.

His recreations were fishing and golf. To the former he was particularly attached, and derived the greatest pleasure from his visits to his fishing in Wales. But the greater part of his scanty leisure was bestowed upon his family; in 1896 he married the eldest daughter of Sir J. Roper Parkington, Bart., and is survived by her, by three sons, and two daughters. He was the most devoted husband and father, and it was in his happy family life that he undoubtedly found his true recreation and pleasure. To them we offer our sincerest sympathy in their sorrow, a sympathy which will be extended to them by many whom they have never known, to whom he has given freely and generously of his skill and knowledge.

Mr. Bidwell leaves a record of a life filled with interest, of good service to the professon he adorned, and of much help given without hope of reward in the best spirit of charity.

Dr. SEYMOUR TAYLOR writes:

Leonard Bidwell has with dramatic suddenness gone to his long rest. It is only when a man has died that his true worth and merit are estimated at their proper value.

Bidwell presented many aspects for our admiration, and imitation if possible. As a surgeon he had few equals in manual dexterity and quickness, and certainly I have known no superiors in these qualities; and yet withal he was a man of great judgement, carefully weighing the risk of an operation, never operating rashly, and never suggesting operation unless such a step held out reasonable hope of a happy termination.

Then also as a teacher he was in the first rank. It did one good to witness the enthusiasm which he brought to his lectures and demonstrations—an enthusiasm which he imparted to his class, if one may judge by the attention which his pupils paid and the assiduity with which they imitated his methods of surgical technique.

As a colleague and friend his qualities were beyond praise. Always kind and considerate towards others, he made quarrels and bickerings impossible, and he was generous almost to a fault in affording his surgical help in any deserving case. There will always exist that difficult patient, who is above the rank of a hospital patient, and yet whose means do not allow of an able surgeon's full fee. Bidwell had only to be told this to secure his services at a lessened remuneration, and I have had many occasions to thank him for such generosity.

In every way he was a remarkable man, and now that he has gone we shall only discover how much the profession, and especially the hospital where he did so great a work, has lost.

Mr. ASLETT BALDWIN writes:

In face of the sudden and unexpected calling away of our friend and colleague, Mr. L. A. Bidwell, one feels scarcely able on the spur of the moment to find words suitable to adequately express all one feels. He was the soul of honesty and truth, a charming and loyal colleague; his example was always a stimulus to those around him to do their best. Always apparently happy and in the best of temper, his cheery presence and help smoothed over many difficulties and lightened many burdens. His loss causes a void in our hospital, our college, and in our hearts which it will be hard, very hard, to fill.

DEPUTY INSPECTOR-GENERAL USHER WILLIAMSON EVANS, A.M.D. (ret.), died at Clifton on August 27th, aged 89. He took the diploma of L.R.C.S.Irel. in 1844, and the degree of M.D.Glas. in the following year. In April, 1846, he joined the service as Assistant Surgeon, becoming Surgeon in 1854, Surgeon-Major in 1866, and retired with the honorary rank of Deputy Inspector-General in 1871. He served in the Crimea, and was present at the battles of Alma, Balaclava, and Inkermann, and the siege of Sebastopol, receiving the British medal with four clasps and the Turkish medal.

SURGEON-GENERAL DUNCAN A. CAMPBELL FRASER, M.D., A.M.S. (ret.), who died on August 28th at Cheltenham, at the age of 80, was the son of the late Rev. H. Fraser, of Ardchaltan, Argyllshire. He qualified as M.D.Edin. in 1853, in which year he joined the service as Assistant Surgeon, becoming Surgeon in 1863, Surgeon-Major in 1873, Brigade Surgeon in 1879, Deputy Surgeon-General in 1881, Surgeon-General in 1887, and retired from the post of P.M.O. at Malta, on reaching the age of 60, in December, 1891. He served in the Ashanti war, 1873–4, and received the medal, and also acted as a Commissioner of the Red Cross Society during the Russo-Turkish war of 1877, receiving the Star of Roumania for his services.

The Services.

INDIAN MEDICAL SERVICE.

THE Secretary of State for India has been pleased to request the Government of India to convey the thanks of His Majesty's Government to Captain W. T. McCowen, I.M.S., for the services he rendered on the occasion of the attack upon Acting Consul Smart near Kazerun in December, 1911.

It has been decided that officers whose commissions are dated September 30th, 1889, are eligible for the extra pensions of £100. Officers whose commissions are dated March 31st, 1890, and later are not eligible.

Universities and Colleges.

UNIVERSITY OF BIRMINGHAM.

IN consequence of the death of the Vice-Chancellor of the University of Birmingham (Alderman C. G. Beale, M.A.), it has been decided, as a mark of respect, to abandon the usual conversazione, which is held at the beginning of October to inaugurate the opening of the medical winter session.

Medico-Legal.

LOCUMTENENT AND FEE FOR LUNACY CERTIFICATION.

P. F. W. asks whether a locumtenent who has certified a lunatic and received a fee for the same while acting for his principal is entitled to retain the fee.

*** As has repeatedly been pointed out in these columns, all fees received by a locumtenent for professional work done on behalf of his principal belongs to the latter. Even if such fees are paid to him personally, it is his duty to hand them over. The only emolument he is entitled to is the stipend which the principal has agreed to pay.

Medico-Ethical.

The advice given in this column for the assistance of members is based on medico-ethical principles generally recognized by the profession, but must not be taken as representing direct findings of the Central Ethical Committee, except when so stated.

THE ETHICS OF ADVERTISING.

J. G.—Such an advertisement would be sure to provoke adverse comment, and the desired result would seem to be quite easily obtained by circulars addressed to the patients in each of the practices it is proposed to combine.

Medical News.

His Majesty the King has granted Dr. Egbert Sumner Verdon licence and authority to accept and wear the Insignia of the Commander of the Hafidian Order, conferred upon him by the Sultan Malai-Abd-el-Hafid of Morocco, in recognition of valuable services rendered.

At the meeting of the Leicestershire Automobile Club at Beacon Hill on August 31st a competition was held among cars made and delivered at any date previous to the Olympia Show of 1907. The winner was a 6-h.p. De Dion delivered in 1902.

The Garden Cities and Town Planning Association (3, Gray's Inn Place, London, W.C.) has recently put itself in a position to send to any part of the country lecturers competent to deal with all aspects of the question denoted by its name, and dates may now be booked by those interested therein.

The King has authorized the following medical officers, for the prevention of plague, Mukden Station, to accept and wear decorations which the Emperor of China has conferred on them in recognition of valuable services rendered: Dr. Dugald Christie, C.M.G., Order of the Double Dragon, Insignia of the First Class of the Third Grade; Dr. Alexander Russell Young and Dr. David Dickson Muir, the Order of the Double Dragon, Insignia of the Second Class of the Third Grade.

The King has, on the recommendation of the Secretary for Scotland, approved the appointment of Dr. Robert Gordon McKerron to be Professor of Midwifery in the University of Aberdeen, vice Professor William Stephenson resigned. Dr. McKerron graduated as M.B., C.M., at Aberdeen with honours in 1888, and in 1898 he was awarded the degree of M.D. with honours, for a thesis on the complication of pregnancy, labour, and childbirth with ovarian tumour. For the past five years he has occupied the post of Physician to the Maternity Hospital, Aberdeen. For nearly twenty years he has held the position of University Assistant to the Professor of Midwifery.

The third Annual Conference of the British Hospitals Association will be held in Birmingham on September 19th and 20th under the presidency of Mr. J. B. Clarke, Chairman of the Board of Management, Birmingham General Hospital. Among the papers to be read are the following: Sir William Collins, Hospitals and the State; Mr. E. S. Kemp, The Training and Work of a Hospital Almoner; Dr. Nathan Raw, The Probable Effect of the Insurance Act on Voluntary Hospitals and other Institutions; Dr. T. Basil Rhodes, The Voluntary Hospitals and the Insurance Act; Mr. J. Danvers Power, Hospital Management Considered with Reference to Responsibility.

The London Inter-Collegiate Scholarships Board will hold an examination in anatomy and physiology on September 24th for entrance scholarships at the medical schools attached to the Westminster, St. George's, Charing Cross, and King's College Hospitals. The candidates must have passed the first M.B. examination of Oxford University or the second M.B. of Cambridge or London, or some other examination accepted as an equivalent of these by the committees of the medical schools concerned. A successful candidate will be required to enter the medical school which has elected him in the following month. Applications for forms of entry may be made to the deans of any of the schools mentioned.

A post-graduate course in connexion with the University of Dublin will begin on September 23rd and last until October 12th. The arrangements, in addition to instruction in all general subjects and special departments, include instruction in anatomy, physiology, and pathology. The hospitals at which the clinical work will be done are within easy reach of the medical school, so little time will be lost in going to and fro. The social side of matters has not been overlooked, arrangements having been made by which those attending the course can reside in rooms in Trinity College and dine in the Hall. The course is open to all qualified practitioners, rooms in college being reserved in order of application. A syllabus can be obtained on application to the Honorary Secretary, 27, Lower Fitzwilliam Street, Dublin.

Letters, Notes, and Answers.

Authors desiring reprints of their articles published in the British Medical Journal are requested to communicate with the Office, 429, Strand, W.C., on receipt of proof.

Telegraphic Address.—The telegraphic address of the EDITOR of the British Medical Journal is *Aitiology, Westrand, London.* The telegraphic address of the British Medical Journal is *Articulate, Westrand, London.*

Telephone (National):—
2631, Gerrard, EDITOR, BRITISH MEDICAL JOURNAL.
2630, Gerrard, BRITISH MEDICAL ASSOCIATION.
2634, Gerrard, MEDICAL SECRETARY.

☞ *Queries, answers, and communications relating to subjects to which special departments of the* British Medical Journal *are devoted will be found under their respective headings.*

QUERIES.

Dr. H. W. Smith (Pleasley, near Mansfield) writes: Can any reader say which book on "Eugenics" is suitable for founding an essay on the subject for an elementary class of laymen?

ANSWERS.

Perplexed.—We are informed that Mrs. Priestly, The Mount, Whitby, Yorkshire, has a school such as is required. We understand that it can be highly recommended.

M.B.—We have made inquiries, but have not been able to learn of a suitable home at such low terms as those mentioned. Possibly the Secretary of the "After-Care Association," Mr. H. Thornhill Roxby, Church House, Deans Yard, Westminster, might, if applied to, be able to assist, being in touch with small homes for convalescents from insanity.

Referee.—Either of the following works would probably meet our correspondent's requirements: *The Medical Examination for Life Assurance*, by F. de Havilland Hall (Bristol, John Wright and Co., 4s.); *Life Insurance and General Practice* (London, Henry Frowde, 7s. 6d.).

H. D. J.—Tests for the Wassermann reaction are made at the Lister Institute, Chelsea Embankment, S.W., the Clinical Research Association, Limited, 15, York Buildings, Adelphi, and the Laboratories of Pathology and Public Health, 38, New Cavendish Street, W. Examinations of the blood for malaria parasites are also made at all these places. Application should be made to the Secretaries.

Venetian Fever.

South African Practitioner, writing in reply to the inquiry of "M." in the Journal of July 6th, makes the same suggestion as Major Kennedy (July 27th), namely, that the fever may be Malta or undulant fever, and suggests if the case has proved stubborn a trial of the *Micrococcus melitensis* vaccine.

LETTERS, NOTES, ETC.

St. Thomas's Hospital.

The honorary secretaries of the old students' dinner at St. Thomas's Hospital this year are Dr. L. S. T. Burrell and Mr. C. M. Page, and not as previously stated.

The Medico-Psychological Association.

The Treasurer of the Medico-Psychological Association of Great Britain and Ireland informs us that the Gaskell Prize is now a gold medal (value 5 guineas) and £45 in cash. A silver medal and a sum of money are occasionally awarded to the *proxime accessit*. The Hack Tuke Memorial Prize is no longer awarded.

Sir Charles Tupper, Bart.

Dr. W. G. Putnam (Yarmouth, Nova Scotia) writes: I have waited for some time to see if the name of Sir Charles Tupper, Bart., were not added to the list of medical baronets mentioned in the Journal of May 25th. It is true his honours came as the result of political and not medical accomplishments. He was made a baronet in 1888. He is an M.D. of Edinburgh of more than sixty years' standing, and is also Hon. LL.D. of Edinburgh, Cambridge, and other universities; is now in his 92nd year, living in Vancouver, B.C., having left England early in the summer.

SCALE OF CHARGES FOR ADVERTISEMENTS IN THE BRITISH MEDICAL JOURNAL.

	£	s.	d.
Eight lines and under	0	4	0
Each additional line	0	0	6
A whole column	2	13	4
A page	8	0	0

An average line contains six words.

All remittances by Post Office Orders must be made payable to the British Medical Association at the General Post Office, London. No responsibility will be accepted for any such remittance not so safeguarded.

Advertisements should be delivered, addressed to the Manager, 429, Strand, London, not later than the first post on Wednesday morning preceding publication, and, if not paid for at the time, should be accompanied by a reference.

Note.—It is against the rules of the Post Office to receive *postes restante* letters addressed either in initials or numbers.